SCIENCE

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AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

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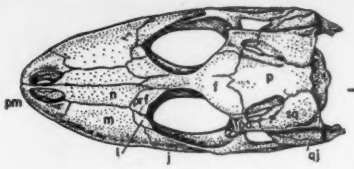

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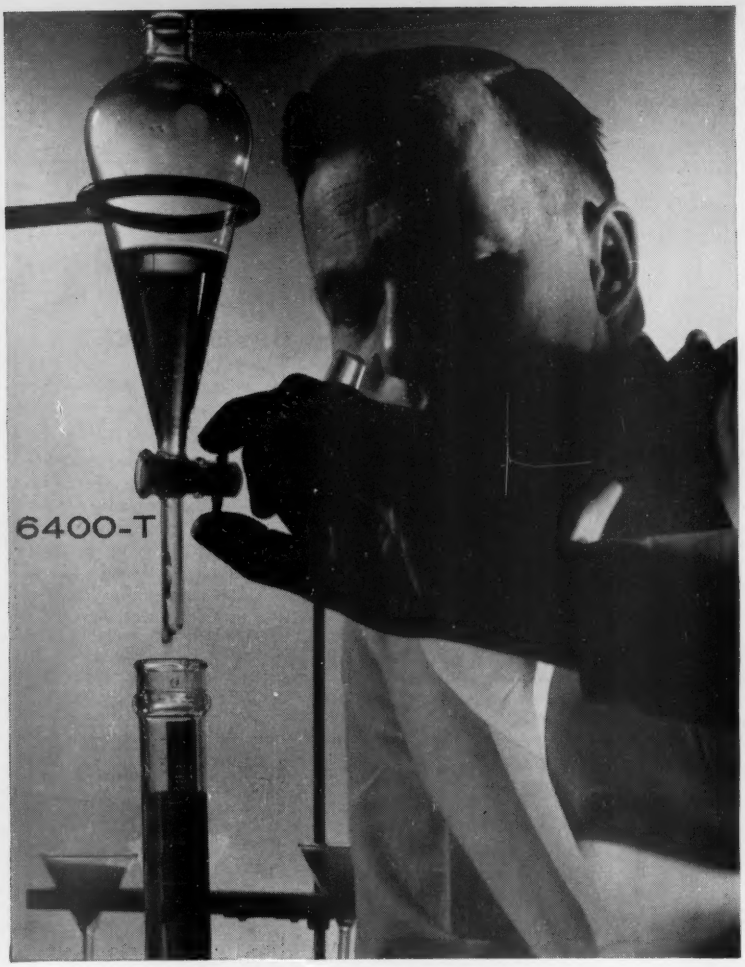

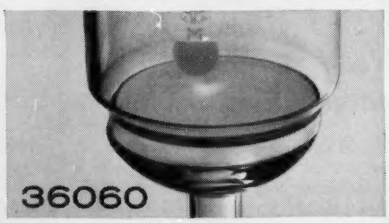

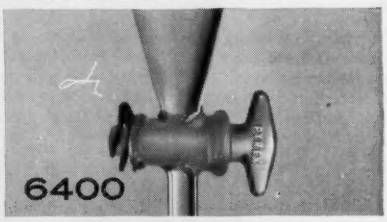

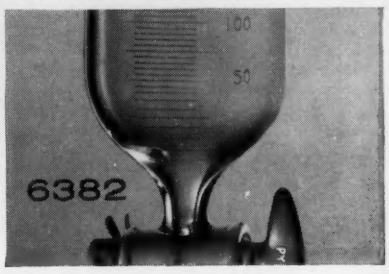

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3 June 1960, Volume 131, Number 3414

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Cover

Electron micrograph of petroleum-bearing strata in the East Texas oil fields (× 60,000) [Courtesy of the American Potash Institute, Inc.]

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About Science

Every week a good many letters, some critical and some laudatory, come into our office. This is all to the good, for it gives us a running impression of reader reaction and stimulates us to decide whether we have sinned and should sin no more or whether we have in fact not sinned at all. But this kind of information is likely to be one-sided: those who are indignant are more inclined to let us know than are those who are pleased.

In order to get a wider and more representative view of reader reaction, we recently sent out a questionnaire to approximately 6200 of our 62,000 subscribers. More than 3500 people have now returned the forms, and of these, more than 2000 wrote short comments to amplify their views. They made scores of suggestions—for possible modification in different sections of the journal, for the introduction of new sections, and for ways in which policy should be determined. Although every shade of opinion was expressed, it is clear that most readers heartily approve the "Current Problems in Research" series of articles, the new type face, the cover design, and the cover picture.

The questionnaire not only tells us how well we are meeting the needs of our readers and what changes might be desirable but also tells us much about our readers. Our composite reader is intelligent, literate, articulate, thoughtful, critical, and constructive. But a composite reader is a convenient fiction; our readers are decidedly individuals. Fifty-three percent of them spend, on the average, an hour each week reading *Science*, and 52 percent pass their copies along to one or more additional readers. On the basis of this information we can estimate that, without counting those who read *Science* in libraries, we have at least 130,000 readers. Eighty-three percent usually read the editorials; 78 percent read the articles; 67 percent, the book reviews; and 63 percent, the technical reports. Doubtless the percentage who read the news falls somewhere in this range, but our questionnaire was poorly designed in some respects, as many pointed out ("next time get a professional to plan your questionnaire") and did not shed much light on reader use of the news section.

We also asked whether each section should be enlarged, left as it is, reduced, or eliminated. "Left as it is" won out in every case, but there were many votes (percentages in parentheses) for enlarging the following: leading articles (20), news (13), book reviews (15), and reports (22).

Few readers shared the view of the one who said that if we needed the kind of advice we asked for we were incompetent and ought to resign. On the contrary, almost all took the opportunity to express constructive suggestions or well-considered criticisms, all of which will be of great help to us in charting our future course. A few typical comments were: "Make no drastic change; one does not like to see a familiar friend change too much too quickly"; "I depend on *Science* to keep me current in fields other than my own"; "I enjoy *Science*, but selectively"; and finally (and there were many like this), "*Science* is fine as it is."—G.DuS.

3 June 1960, Volume 131, Number 3414 SCIENCE

Demographic Dimensions of World Politics

Population explosion has implications for the conflict between the free world and the Communist bloc.

Philip M. Hauser

Politics in general, as well as world politics, is a branch of engineering—social engineering—not of science. Yet the consideration of the demographic aspects of world politics is not an inappropriate subject for a scientific journal. It is the purpose of this article to point to ways in which the findings of the science of demography illuminate various aspects of the world political scene.

There are various ways in which this subject can be developed, but I have arbitrarily chosen to discuss population factors in relation to politics, broadly conceived, on the global and on the international levels, respectively. By "global" problems I mean those that concern the earth as a whole; by "international" problems I mean those that arise among the various political subdivisions of the globe.

Global Considerations

There is no world government charged with the task of achieving world order and performing other civil governmental functions for the earth as a whole. This, however, does not mean that there are no political problems of a global, as distinguished from an international, character. Some such global problems are in fact dealt with by the United Nations and its specialized agencies, which are, of course, organizations of individual sovereign nations rather than organs of world government. Examples of global problems—problems which transcend and cannot be contained within national boundaries—include health, weather, fallout, and the newly emergent problems of outer space. It is easy to demonstrate that the contemporary rate of world population growth also constitutes a global problem—one which would be of great concern to a world government if we had one, and one which is of increasing concern to various organs of the United Nations and the specialized agencies.

Although the first complete census of mankind has yet to be taken, it is possible to reconstruct, with reasonable accuracy, the history of world population growth. This history may be encapsulated in the following estimates of the population of the earth: at the end of the Neolithic period in Europe (8000 to 7000 B.C.) (*1*), 10 million; at the beginning of the Christian era, 200 to 300 million; at the beginning of the modern era (1650), 500 million; in 1950, 2.5 billion.

These four numbers constitute a measurement of one of the most dramatic aspects of man's existence on the globe, and they explain the purple language of the demographer in describing the changes in rates of population growth during the modern era as a "demographic revolution" or "population explosion" (*2*).

The basis for the demographer's emotionally surcharged language may be summarized as follows.

1) The present population of the world could be produced from an initial population of two dozen individuals increasing at the rate of 0.02 percent per year over a period of 100,000 years, and man has been on the earth for at least 200,000 to 1 million years.

2) The rate of population growth has increased enormously over the three centuries of the modern era (1650–1950), during which time it averaged about 0.5 percent per year. Over this period the rate of growth increased from about 0.3 percent per year between 1650 and 1750 to 0.9 percent per year between 1900 and 1950. World population growth averaged 1 percent per year between 1930 and 1940.

Now, a 1-percent return per year, even compounded, would by our standards represent a meager return on investment. But it constitutes a fantastically rapid rate of population increase. One hundred persons multiplying at 1 percent per year, not over the period of 200,000 to 1 million years of man's occupancy of this globe but merely for the 5000 years of human history, would have produced a contemporary population of 2.7 billion persons per square foot of land surface of the earth! Such an exercise in arithmetic, although admittedly dramatic and propagandistic, is also a conclusive way of demonstrating that a 1 percent per year increase in world population could not have taken place for very long in the past; nor can it continue for very long into the future.

The demographer's concern is not based only on considerations of the

The author is professor and chairman of the department of sociology, University of Chicago. This article is adapted from his vice-presidential address to Section K of the AAAS, delivered 27 Dec. 1959 during the Chicago meeting.

Table 1. Population, income, and energy consumed per capita, by continent, about 1950. Source of data, United Nations, except where otherwise indicated.

Area	Total population		Aggregate income		Per capita income ($)	Energy consumed per capita (kw-hr)†
	No. (thousands)	(%)	Dollars* (millions)	(%)		
World	2497	100.0	556	100.0	223	1676
Africa	199	8.0	15	2.7	75	686
North America	219	8.8	241	43.3	1100	10,074
South America	112	4.5	19	3.4	170	741
Asia	1380	55.3	69	12.4	50	286
Europe (exclusive of U.S.S.R.)	393	15.7	149	26.8	380	3117
U.S.S.R.	181	7.2	56	10.1	310	1873
Oceania	13	0.5	7	1.3	560	3543

* See (*8*, *9*). † See (*33*).

past. It is even more justified by postwar developments in population growth.

Since the end of World War II the rate of population increase has continued to accelerate and has reached a level of about 1.7 percent per year. There is justification, indeed, for pointing to a new population explosion in the wake of World War II of a greater magnitude than that previously observed. At the rate of world population increase for the period 1800–1850, for example, the present population would double in 135 years; at the 1900–1950 rate, in 67 years; and at the postwar rate, in only 42 years.

Projection of the post-World War II rate of increase gives a population of one person per square foot of the land surface of the earth in less than 800 years. It gives a population of 50 billions (the highest estimate of the population-carrying capacity of the globe ever calculated by a responsible scholar) in less than 200 years! This estimate, by geochemist Harrison Brown (*3*), is based on the assumptions that developments in the capturing of solar or nuclear energy will produce energy at a cost so low that it would be feasible to obtain all the "things" we need from rock, sea, and air, and that mankind would be content to subsist largely on food products from "algae farms and yeast factories!"

Moreover, the United Nations estimates of future world population indicate even further acceleration in the rate of world population growth during the remainder of this century. Between 1950 and 1975 the average annual percentage of increase, according to the United Nations "medium" assumptions, may be 2.1 percent, and between 1975 and 2000, almost 2.6 percent (*4*). Such rates of increase would double the population about every 33 and 27 years, respectively.

It is considerations of this type that would make it necessary for a world government to exercise forethought and planning, which constitute rational decision making, in facing the future. This, of course, is the purpose of the projections. The figures do not show what the future population of the world will be—for the world could not support such populations. They do demonstrate that man, as a culture-building animal, has created an environment in which the rhythm of his own reproduction has been modified in such a manner as to point to crisis possibilities.

Crisis Possibilities

The crisis possibilities are of several forms, each posing major world political problems. The first, we may note, is the ultimate crisis, which would result from the fact that the globe is finite (*5*) and that living space would be exhausted. Unless one is prepared to argue that future technological developments will enable man to colonize other globes (*6*), it is clear that present rates of population increase must come to a halt by reason of lack of space. No facts or hopes as to man's ability to increase his food production and to increase other types of goods and services can indefinitely increase man's *lebensraum* (or could do so even if we accept the absurd assumption that man, at terrific cost, could burrow into the earth, live in man-made layers above it, or live on the seas).

In the short run, let us say to 1975 or to 2000, world population will be confined to much more manageable numbers. The United Nations projects, on the basis of its medium assumptions, a world population of about 3.8 billion by 1975 and 6.3 billion by 2000 (*1*, p. 23).

In the short run there is no problem of exhausting the space on the globe, nor is there reason to fear serious decreases in world per capita food supply, as is evidenced by projections of The Food and Agricultural Organization and others concerning foodstuffs (*7*). But there is great reason to be pessimistic about the possibility of greatly increasing the average world level of living during the remainder of this century.

In 1950, world per capita income was estimated at $223 (*8*, *9*). In North America, per capita income was $1100. Had each person on the globe enjoyed the North American level of living in 1950, as measured by per capita income, the aggregate world product in 1950 would have supported only 500 million persons, as contrasted with the actual world population of 2.5 billion. For average world income to have matched income in North America, aggregate income would have had to be increased about fivefold. To bring world per capita income by 1975 to the level enjoyed in North America in 1950 would require about a 7.5-fold increase of the 1950 level in 25 years. To do the same by 2000 would require a 12-fold increase in the 1950 world income within 50 years.

Even if the more modest income level of Europe ($380 per capita in 1950) were set as the target, great increases in productivity would be necessary, because of prospective rates of population increase, to raise average world income to the required level by 1975 or 2000. To achieve this goal by 1975, world income would have to be increased 2.5-fold over the 1950 level, and to achieve it by 2000, the required increase would be greater than fourfold. A decline in the rate of world population growth to that of the period 1800 to 1850—namely, to 0.5 percent—would decrease by three-fourths and four-fifths, respectively, the projected world-income requirements for attaining this goal by 1975 or 2000.

These considerations not only show the enormous difficulty of materially increasing the world level of living on the basis of present rates of population increase but indicate, also, the weakness of the argument that a solution to the population problem is to be found in more equitable distribution of the world's food supply or of goods and services in general (*10*). The equitable distribution of world income in 1950 would, to be sure, have raised the per capita income of Latin America by 31 percent; of Africa, almost threefold,

and of Asia, four- to fivefold, but it would still have produced a per capita income per annum of $223, only one-fifth that in North America and only three-fifths that in Europe (exclusive of the U.S.S.R.). The miserably low level of living of most of the world's population is attributable not so much to maldistribution as to low aggregate product, the result of the low productivity of most of the world's peoples.

These political problems of a global character may perhaps be better understood through consideration of their international aspects, special attention being given to the plight of the two-thirds of the world's population resident in the underdeveloped areas of the world, in Asia, Africa, and Latin America.

Table 2. Estimated population and population increases, by continent, 1900 to 2000 (*4*).

Area	Population (million)					Av. annual increase (%)*			
	1900	1925	1950	1975	2000	1900–1925	1925–1950	1950–1975	1975–2000
World	1550	1907	2497	3828	6267	0.9	1.2	2.1	2.6
Africa	120	147	199	303	517	0.9	1.4	2.1	2.8
Northern America	81	126	168	240	312	2.2	1.3	1.7	1.2
Latin America	63	99	163	303	592	2.3	2.6	3.4	3.8
Asia	857	1020	1380	2210	3870	0.8	1.4	2.4	3.0
Europe (including U.S.S.R.)	423	505	574	751	947	0.8	0.6	1.2	1.0
Oceania	6	10	13	21	29	2.3	1.4	2.4	1.6

* Arithmetic mean of percentage of increase for 25-year periods.

International Considerations

The short-run implications of present rates of world population growth are manifest in specific forms and in varying degrees of intensity among the various regional and national subdivisions of the globe. The distribution of the world's population and of the world's utilized resources, manifest in differentials in levels of living, is the result, of course, of millenia of human history. The demographic dimensions of international politics may best be comprehended against the background of differences among peoples in levels of living and the significance of these differences at this juncture in world history (*8, 11, 12*) (Table 1).

To note the extremes, North America in 1950, with about 16 percent of the earth's land surface, contained less than 9 percent of the world's population but about 43 percent of the world's income. Asia, in contrast, with about the same proportion of the world's land surface (18 percent), had 55 percent of the world's population but only 12 percent of the world's income. Per capita income in Asia was at a level of about $50 per year as contrasted with a level of $1100 in North America. Despite the fact that such comparisons are subject to considerable error (*13*), there is no doubt that a tremendous difference in per capita income existed, of a magnitude perhaps as great as 20 to 1.

The major factor underlying this difference is indicated by the contrast in the difference in nonhuman energy consumed in North America and Asia, respectively—over 10,000 kilowatt-hours per capita per year for the former in contrast to less than 300 for the latter. The availability of nonhuman energy for the production of goods and services is perhaps the best single measurement available of differences in capital investment, know-how, and technology which account for the great differences in productivity and, consequently, in the size of the aggregate product available for distribution.

The other relatively underdeveloped continents of the world also had relatively low shares of world income as compared with their proportions of world population. Africa, with a per capita income of about $75 per year, and South America, with $170, were also well below not only the level for North America ($1100) but also the levels for Europe (exclusive of the U.S.S.R.) ($380), the U.S.S.R. ($310), and Oceania ($560). There is a high correlation among these areas between per capita income and amount of nonhuman energy consumed (Table 1).

These differences in levels of living, as it turns out, are in general inversely related to present and prospective rates of population increase. The populations of the relatively underdeveloped continents of the world are increasing at a more rapid rate than those of the economically advanced continents (*4, 14*) (Table 2). Between 1950 and 1975, to use the medium projections of the United Nations, while the population of Northern America is increasing at an average annual rate of 1.7 percent and that of Europe, at 1.2 percent, that of Asia will be growing at an average annual rate of 2.4 percent, that of Africa at 2.1 percent, and that of Latin America at 3.4 percent. Between 1975 and 2000, while the rate of increase for Northern America will average 1.2 percent per year and that for Europe, 1.0 percent, the rate for Asia will be 3.0 percent, that for Africa 2.8 percent, and that for Latin America 3.8 percent, a rate at which the population would double about every 18 years.

As I have indicated above, rapid increase in world population imposes a severe burden on efforts to raise levels of living. It is easy to demonstrate that the burden would become an impossible one for the economically underdeveloped areas should their rates of population increase follow the trends indicated in the United Nations projections.

For example, Asia, merely to maintain her present low level of living, must increase her aggregate product by 60 percent between 1950 and 1975, and by an additional 75 percent between 1975 and 2000. To raise her per capita income to the European level for 1950 while continuing to experience her rapid population growth, Asia would have to increase her 1950 aggregate income 12-fold by 1975 and 21-fold by 2000. Africa, to do the same, must increase her aggregate income eight-fold by 1975 and 13-fold by 2000, and Latin America would have to increase her aggregate income fourfold by 1975 and eightfold by 2000 (*15*).

To achieve a per capita income equal to that of Northern America in 1950 while experiencing the projected population growth, Asia would have to increase her aggregate income 35-fold by 1975 and 62-fold by 2000. Africa, to achieve a similar goal, would require 22-fold and 38-fold increases, respectively, in aggregate income, and Latin America, 12-fold and 23-fold increases.

These considerations provide additional justification for the use by the demographer of the phrase *population explosion;* and they certainly indicate the hopeless task which confronts the underdeveloped areas in their efforts to achieve higher levels of living while experiencing rapid population growth. The control of rates of population growth would unquestionably decrease

Table 3. Summary of projections of urban population for the world and for Asia, 1975 (*18*).

Cities (category)	Population (millions)			Estimate of increase in population, 1950–1975 (millions)		Estimate of increase in population, 1950–1975 (%)		Proportion of total population in cities	
	Projection for 1975		1950					Projection	
	Upper	Lower		Upper	Lower	Upper	Lower	1975*	1950
				The world					
100,000 and over	745	488	314	431	174	138	55	19	13
20,000 and over	1155	779	502	653	277	130	55	30	21
				Asia					
100,000 and over	340	176	106	234	70	222	66	15	8
20,000 and over	544	283	170	374	113	220	66	25	13

* Figures are based on the "upper" projection, which assumes urbanization of an increasing proportion of the population.

the magnitude of the task of achieving higher levels of living in the underdeveloped areas, especially in those with populations that are large relative to resources (*16*).

Increasingly large proportions of the population in the underdeveloped areas of the world are becoming concentrated in urban places. The continued acceleration in the rate of world urbanization during the first half of this century was mainly attributable to urbanization in the underdeveloped areas, which proceeded at a pace considerably above that in the developed areas (*17*). I have had occasion to make projections of the urban population of the world and of Asia to 1975; these are presented in Table 3 as illustrative of what is in prospect in the underdeveloped areas of the globe (*18*). For the rate of urbanization in Latin America and Africa is, also, accelerating.

The projections for Asia indicate that in the 25 years between 1950 and 1975, in cities either of 100,000 and over or of 20,000 and over, urban population will increase by at least two-thirds and may perhaps triple. The lower projection is based on the assumption that the proportion of urban population in Asia will be the same in 1975 as it was in 1950. Under this assumption the projected increase would result from total population growth alone. But if it is assumed that the rate of urbanization in Asia will increase as it did between 1900 and 1950 while the total population continues to grow at the rate projected by the United Nations, then tripling of Asia's urban population is indicated.

Thus, while the nations of Asia are attempting to improve their miserable urban living conditions, their urban populations will continue to increase explosively—perhaps to triple within a period of less than one generation.

In the economically more advanced nations of the world, urbanization is both an antecedent and a consequent of technological advance and of a high level of living—a symbol of man's mastery over nature. In the underdeveloped nations, however, urbanization represents instead the transfer of rural poverty from an over-populated and unsettled countryside to a mass urban setting. In the economically underdeveloped areas of the world, urbanization is outpacing economic development and the city is more a symbol of mass misery and political instability than of man's conquest of nature (*17, 19*).

The prospect for individual nations, while variable, is in general the same—one of explosive growth. Between 1955 and 1975, according to the United Nations medium projections, the population of China will increase by 294 million persons and that of India, by 177 million (*4, 20*). That of Pakistan will increase by 45 million persons, and that of Indonesia, by 40 million, in these 20 years. Japan, although she has now greatly slowed down her rate of population growth, will, despite her already great population pressure, increase by an additional 27 million. To confine our attention to the Far East for the moment, smaller countries with the most explosive increases include South Korea, Taiwan, and Ceylon. Each of these nations is faced with a task of tremendous proportions merely to maintain her present level of living, let alone to greatly increase it while continuing to grow at the projected rates.

Political Instability

What will happen if the underdeveloped areas in Asia are frustrated in their efforts to attain a higher standard of living?

Warren S. Thompson devotes his latest book to providing an answer to this question (*21*). The larger of these nations are not apt to remain hungry and frustrated without noting the relatively sparsely settled areas in their vicinities—the nations in the South-East Asian peninsula: Burma, Thailand, and the newly formed free countries of Indochina, Laos, Cambodia, and Vietnam. (Vietminh, that is North Vietnam, is already engulfed by Communist China.) Even parts of thinly settled Africa may be subject to the aggressive action of the larger and hungrier nations as feelings of population pressure mount. Moreover, Communist China, the largest nation in the world by far, faced with the greatest absolute population increases to add to her already heavy burdens in striving for economic development, may not confine her attention only to the smaller nations within her reach. Her present actions relative to her boundaries with India and possible tensions over her boundaries with the U.S.S.R. contain explosive possibilities.

It is Thompson's conclusion that the larger nations in the Far East, including Japan, India, and Pakistan as well as China, may resort to force to achieve access to additional resources under sufficient population pressure. The smaller countries may not be able to resort to force but are almost certain to require outside aid to prevent chaos. Furthermore, while neither Indonesia nor the Philippines is in a position to be aggressive or is easily accessible to aggressors, both, under mounting population pressures, are likely to continue to experience growing internal political instability.

Population pressure as a factor in political instability is not confined to the Far East. Populations of the Middle East and North Africa—the Muslim area (exclusive of Pakistan)—may increase from 119 million in 1955 to 192 million by 1975, an increase of 73 million or 61 percent in 20 years (*4*). As Irene Taeuber has noted, this is an area "where internal instabilities and conflicts of religious and ethnic groups create recurrent crises for the region and world." Taeuber observes that the immediate political instabilities in this area are attributable more to "diversities among the peoples and the nations than to population pressure or population growth" (*22*). But she points to the importance, in the decades that lie ahead, of economic advances to lessen tension in this region and to the barrier

that rapid population growth may contribute to that development.

Latin America, although in large part still a sparsely settled area of the world, is already experiencing problems associated with rapid population growth which give promise of worsening. For Latin America, as has been reported above, is faced with a population increase of 86 percent between 1950 and 1975 and of 95 percent, almost a doubling, between 1975 and 2000 (*4, 23*). Especially difficult in Latin America are the problems posed by accelerating rates of urbanization. Recent measurements of rate of urban growth in Latin America indicated that of 15 countries for which data were available, urban population in one, Venezuela, was increasing at 7 percent per year, a rate which produces a doubling about every 10 years; seven had growth rates which would double their population in less than 18 years; and only two (Chile and Bolivia) had rates of urban growth of less than 1 percent per year (*19, 24*). Growth rates (total and urban) of the magnitude which Latin America is experiencing are likely to add appreciably to the difficulty of raising living levels and are likely to worsen already existent political instabilities that threaten internal order and may affect world peace.

Finally, a fourth region of political instability to which the population factor is a contributing element, and one where it will be increasingly manifest, is sub-Saharan Africa (*22, 25*). Middle Africa is sparsely settled, but increasing knowledge about the area indicates high birth rates, decreasing death rates, and explosive growth. The United Nations projections indicate a population increase from 154 million in 1955 to about 202 million in 1975, or an increase of 31 percent. The familiar syndrome of underdeveloped areas—malnutrition, disease, and urban and rural squalor on the one hand and aspirations for independence and economic development on the other—are now emergent in this most primitive continent of the globe. And here, as in the other underdeveloped areas, rapid population growth is likely to intensify political unrest.

In southern Africa another type of population problem is also a major element in a political problem that has grave implications for world order as well as for the stability of the Union of South Africa. This is the problem arising from the conflict between the indigenous people and European settlers manifest in apartheid. Rapid and differential rates of growth of native and European populations are likely to intensify rather than to allay conflict in southern Africa.

The tensions and political instabilities generated by explosive population growth in the economically underdeveloped nations have a special significance in the contemporary world, characterized by the bipolar conflict between the Free and Communist blocs and the efforts on the part of each to win the allegiance of the uncommitted nations of the world. This conflict has several demographic dimensions of importance.

The Free and Communist Blocs

The first of these dimensions is evident in the way in which population is distributed among the three political blocs into which the world is divided. For in 1955, each of these political groups—the free nations, the Communist nations, and the uncommitted nations—had approximately the same population. The Free and the Communist blocs, respectively, each have much to gain in the struggle to win the allegiance of the uncommitted third of the world's people. This titanic competition is focused primarily on South and Southeast Asia at the present time, because the bulk of the world's politically uncommitted population is located there.

In this war for men's minds, the competition between free-world and Communist ideologies, each of the contestants has powerful weapons. Apart from military power, which I will leave out on the assumption that a nuclear stalemate exists, the key weapons of the Communists, as is daily attested to by their propaganda, are the exploitation of the wide gap between the levels of living of the "have" and "have-not" nations and the attribution of blame for the misery of the "have-not" nations on the imperialistic and colonial practices of the "have" powers. Needless to say, the fire of this propaganda is effectively fed by the frustration of the underdeveloped areas in their efforts to advance their levels of living, or in their efforts to win independence from imperial powers, where this is not yet accomplished.

The Communist bloc, with relatively little, but with increasing, surplus product, is attempting more and more to help the uncommitted nations in economic development. The U.S.S.R. may perhaps be departing from its postwar cold-war policy of trying to persuade uncommitted nations to accept its ideology by means either of internal coups or direct external aggression.

The chief weapon of the free nations, apart from the example of their free way of life, is, undoubtedly, the provision of assistance to the underdeveloped nations to help them achieve their economic goals.

Thus, the success or failure of underdeveloped areas to raise their levels of living has the most profound world political implications. The most important immediate international political question is the question of whether the free-world approach or the Communist approach is the more effective one for achieving economic development.

It is to be emphasized that this is not a rhetorical or hypothetical question. It is being answered by the course of events, the definitive test of achievement. It is being answered by what may be regarded as the most important experiments of all time—experiments under way in each of the three blocs of nations. A great race is on among the economically underprivileged nations to attain higher living levels—some by relatively free, and some by totalitarian and Communist, methods. The contests involve nations within each of the great political blocs, for within each of them both economically advanced and underdeveloped areas are to be found (*26*).

The greatest single race under way is undoubtedly the race between the leaders of the Free and Communist blocs, respectively—that is, the United States and the U.S.S.R. The U.S.S.R. has certainly served notice that, by its methods, it hopes to surpass the level of living attained by the United States, and in the not too distant future. Overshadowed only by the direct contest between the United States and the U.S.S.R. is the race between India and Communist China (*27*), a race of special and direct immediate interest to the underdeveloped areas. For these mammoth nations, the two largest in the world, are bending every effort to achieve higher living standards—one through the Communist approach and the other by democratic methods. The outcome of this race will be of great interest not only to the underdeveloped nations in the uncommitted bloc but also to those in the Free bloc—the underdeveloped nations in Latin America as well as those committed to the Free bloc in Asia and in Africa.

The international political situation, then, as described above, gives a special significance to explosive population growth. For present and future rates of population growth may, indeed, prevent underdeveloped nations from raising their levels of living. Simon Kuznets' examination of the evidence indicates that the gap between "have" and "have-not" nations is increasing rather than decreasing (*12*). To the extent that underdeveloped nations are frustrated in their efforts to advance their living standards, they will, it may be presumed, be more open to the blandishments of the Communist bloc. Furthermore, if the underdeveloped Communist nations demonstrate that they can achieve more rapid economic progress than the underdeveloped free nations, the free way of life may well be doomed. Success or failure in this fateful contest may well hinge on the ability of the nations involved to decrease their rates of population growth (*28*).

The Alternatives

The "why" of the population increase, in an immediate sense, is readily identifiable. It is to be found in the great increase in "natural increase"—in the gap between fertility and mortality (*1*). Quite apart from the precise timing of changes in the relations between mortality and fertility, it is clear that explosive growth can be dampened only by decreasing natural increase. This is true for the world as a whole in the ultimate sense, with differences in timing for different parts of the world. For suggested solutions to the problems of present and prospective rates of population growth in the various subdivisions of the world through migration, foreign trade, redistribution of wealth, and similar means hold forth little promise, if any, even in the short run (*21*, chap. 18).

There are only three ways to decrease natural increase: (i) by increasing the death rate; (ii) by decreasing the birth rate; and (iii) by some combination of the two.

Although it is true that decreased death rates were largely responsible for the population explosion in the past and are foreseen to be a large factor in the future, the adoption of a policy to increase mortality, or to diminish efforts to increase longevity, is unthinkable. Unless one is prepared to debate this, two of the three ways of decreasing natural increase are ruled out. For two of them involve an increase in death rates.

If longevity gains are to be retained, then, the only way to reduce explosive population growth is to decrease the birth rate. That is, the "death control" mankind has achieved can be retained only if it is accompanied by birth control. This proposition, even though it flows directly from the demographic facts of life, in view of prevalent value systems provokes heated debate of the type manifest in the press. Birth control has recently, indeed, made the front pages of the world press.

What is important about the value controversy under way is that it definitely affects global and international policy and action on matters of population and, therefore, on the crucial political problems involved. The most significant thing about all the available methods of birth control—a fact mainly obscured in the present public controversy—is that they are by no means adequate to the task of slowing down explosive world population increase, especially that in the underdeveloped areas. The great mass of mankind in the economically less advanced nations which are faced with accelerating rates of growth fail to limit their birth rates not because of the factors at issue in the controversy we are witnessing but because they do not have the desire, the know-how, or the means to do so. The desire to control fertility, arising from recognition of the problem, is, however, increasing. Japan is already well down the road to controlling its birth rate, although by methods which are not enthusiastically endorsed either by the Japanese themselves or by other peoples. China, India, Pakistan, and Egypt (*29*) have population limitation programs under way or under serious consideration, and other underdeveloped areas are showing increasing interest in this problem (*30*). The changes in value systems which will create mass motivation to adopt methods of family limitation are not easily brought about (*31*), but they are at least under way.

Birth control methods in use in the economically more advanced nations are not, in the main, well adapted for use in the underdeveloped areas. But the results of increased research and experimentation with oral contraceptives are encouraging (*32*), and there may soon be a breakthrough on obtaining adequate means for the task of limiting population growth in the underdeveloped areas.

Conclusion

The demographer and the increasing number of his allies, in directing attention to the implications of world population growth, are in fact pointing to major global and international political problems—problems that cannot be ignored. Needless to say, the solution to the problems is not to be found in appeals to the traditions of the past, sacred or secular. The solution is to be found in the policies and actions which man himself, as a rational animal, must work out and implement. The mind of man, which has conceived remarkable methods for increasing life expectancy, is probably ingenious enough to devise methods by which the population explosion can be controlled within the framework of man's diverse value systems.

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3. H. Brown, *The Challenge of Man's Future* (Viking, New York, 1954).
4. *The Future Growth of World Population* (United Nations, New York, 1958).
5. This fact is ignored by Roman Catholic bishops [see New York *Times* (26 Nov. 1959)] and by the Pope [see "Pope denounces birth limitation," New York *Times* (15 Dec. 1959)].
6. The impracticability of colonizing other planets is considered by G. Hardin [*J. Heredity* **50**, 2 (1959)].
7. W. H. Leonard, *Sci. Monthly* **85**, 113 (1957).
8. "National and Per Capita Income of 70 Countries in 1949," *U.N. Statist. Papers, Ser. E, No. 1* (United Nations, New York, 1950).
9. The calculations were made by using United Nations per capita income figures for each continent applied to revised United Nations estimates of 1950 population of continents to obtain revised aggregate income by continent and for the world, as shown in Table 1. A new world per capita figure of $223 was obtained, as compared with the published figure of $230.
10. For the Communist position see F. Lorimer, "Population policies and politics in the Communist world," in *Population and World Politics*, P. M. Hauser, Ed. (Free Press, Glencoe, Ill., 1958); for the Catholic position see "Pope denounces birth limitation," New York *Times* (15 Dec. 1959); for the Socialist position, see J. D. Bernal, "Population growth is no threat for a free society," *Natl. Guardian* (7 Dec. 1959) (extract from J. D. Bernal, *Science in History*).
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12. S. Kuznets, "Regional economic trends and levels of living," in *Population and World Politics*, P. M. Hauser, Ed. (Free Press, Glencoe, Ill., 1958).
13. *Report on International Definition and Measurement of Standards and Levels of Living* (United Nations, New York, 1954).
14. Note the different definitions of area in Tables 1 and 2. In Table 2, which gives population projections to 1975 and 2000, "Northern America" includes only North America north of the Rio Grande; "Latin America" includes South America, Central America, and North America south of the Rio Grande. For the rough comparisons made, no adjustment of the data was necessary.
15. Calculations were based on revised data, as explained in (*9*). For Latin America the

calculations were based on a comparison of estimated aggregate income for "Latin America" in 1950, per capita income for "South America" being used.

16. The "population problem" differs for areas with different ratios of population to resources; for example, see Political and Economic Planning, *World Population and Resources* (Essential Books, Fairlawn, N.J., 1955).
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26. K. Davis, "Population and power in the free world," in *Population and World Politics*, P. M. Hauser, Ed. (Free Press, Glencoe, Ill., 1958).
27. W. Lippmann, "China is No. 1 problem," Chicago *Sun-Times* (14 Dec. 1959); "To live India must change its way of life. . . ," Chicago *Sun-Times* (15 Dec. 1959).
28. Nor is population a factor in political instability only in the underdeveloped areas. There are many other demographic dimensions of world politics which cannot be treated here because of limitations of space. The authors of a recent symposium volume which it was my privilege to edit include further considerations of population as a factor in world politics. Especially pertinent are the articles by Kingsley Davis, Frank Lorimer, Irene Taeuber, and Quincy Wright, from which I have drawn material for this discussion.
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Sir Francis Simon

Knowledge of the properties of matter at temperatures near absolute zero has been increased greatly by his work.

P. W. Bridgman

Sir Frances Simon was born on 2 July 1893 in Berlin, the only son (he had two sisters) in a well-to-do family. He attended the Kaiser Friederich Reform Gymnasium and at first devoted himself to the classics, at the wish of his grandfather. The classics he did not like at all. He showed extraordinary talent for physics and mathematics, and it is said that it was under the influence of Michaelis of the Rockefeller Institute of Medicine in New York, an old friend of the family, that he decided to become a scientist, at the age of 14. Michaelis also persuaded his family, after considerable initial opposition, that this was the proper step. Simon graduated from the Gymnasium in 1912. After this he briefly attended the universities of Göttingen and Munich, until he was called into the army in the fall of 1913 for his year of military service. World War I broke out before his year was up, and he continued in the army, connected with the field artillery, for the duration of the war. He was badly affected by gas, and was twice wounded, the second time severely; two days before the armistice. He was confined to a military hospital until the summer of 1919, recovering from his injuries, and he thus lost altogether six years at the beginning of his scientific career. For his services in the war he was awarded the Iron Cross, first class, and was also made an officer—two unusual distinctions for a person of Jewish origin.

In 1919 Simon resumed his studies, now at the University of Berlin, working under Nernst on specific heats at low temperatures, a topic of great interest at the time because of its bearing on Nernst's controversial heat theorem. In 1921 he was awarded the degree of Dr. Phil. in *physics* under Nernst, the physical *chemist*. His other teachers at Berlin included Planck, von Laue, and Haber. In 1922 he was appointed Nernst's assistant at the Physikalische Chemische Institut. In this same year he married Charlotte Münchhausen, also of a well-to-do Berlin family, whom he had met socially two years before. Her tastes were artistic and musical, and she and Simon complemented each other perfectly. There can be no doubt but that his scientific effectiveness owed much to his happy family background (*1*). In 1924 Simon was appointed *Privat Dozent* in physics in Berlin, and in 1927, *Ausserordentlicher* (associate) professor in physics, a post which he held until he left for Breslau in 1931.

Early Publications

Simon's first published paper, on specific heats at low temperatures, appeared in 1922. During the nine years of his stay in Berlin he published 50 papers altogether; these 50 papers foreshadow most of his later scientific activity, and nearly all were connected in some way with low temperatures. At first he continued the work he had done while with Nernst—namely, on specific heats at low temperatures and, in particular, on the various anomalies at the lowest temperatures. These anomalies vitiated a smooth extrapolation of results at higher temperatures and they had to be taken into account in calcu-

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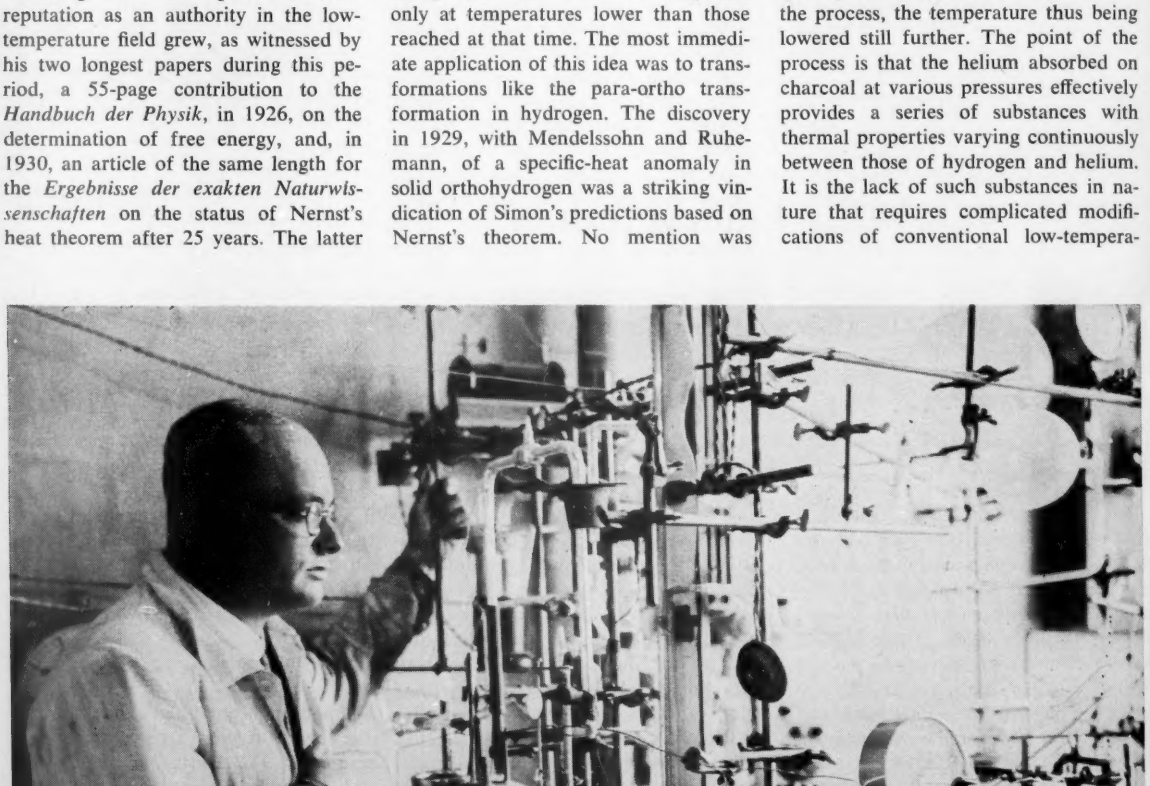
lating entropies at very low temperatures in checking the validity of Nernst's heat theorem. The quantum origin of many of these anomalies was recognized and discussed. Because of their bearing on the same question, the "chemical constants" of the monatomic gases and of mercury were determined. Also discussed was the equivalence of the Nernst theorem with the thesis of the unattainability of absolute zero. A paper had appeared on zero point energy, a result of quantum theory, which was recognized and accepted but which, rather surprisingly, did not seem to play a very vital role in Simon's picture of the limiting condition of physical systems at absolute zero as one of absolute order.

During his Berlin period Simon's reputation as an authority in the low-temperature field grew, as witnessed by his two longest papers during this period, a 55-page contribution to the *Handbuch der Physik*, in 1926, on the determination of free energy, and, in 1930, an article of the same length for the *Ergebnisse der exakten Naturwissenschaften* on the status of Nernst's heat theorem after 25 years. The latter paper is of importance as foreshadowing his later, mature attitude toward the Nernst theorem. Simon recognized the frequent occurrence of anomalies in the specific heats at low temperatures, which must be determined experimentally in order to make good calculations of the equilibrium parameters. He also recognized the existence of phases not well defined thermodynamically, but he regarded the error introduced by these factors in the calculation of equilibrium constants as of only minor importance. It is interesting that among sources of anomalies in the low-temperature specific heats the onset of order in those rotational states which play a role in molecular spectra was recognized as a possibility, although this onset was to be expected only at temperatures lower than those reached at that time. The most immediate application of this idea was to transformations like the para-ortho transformation in hydrogen. The discovery in 1929, with Mendelssohn and Ruhemann, of a specific-heat anomaly in solid orthohydrogen was a striking vindication of Simon's predictions based on Nernst's theorem. No mention was made at this time of magnetism. From this distance it seems that a connection with the phenomena of magnetism is most natural, and that here was a narrowly missed opportunity to propose the magnetic method of cooling, first proposed a few years later.

During the Berlin period Simon began to develop techniques for reaching liquid helium temperatures on a small scale — techniques suitable for the smaller laboratories which lacked the elaborate equipment of installations such as those at Leyden. His first proposal along these lines was the "desorption" method, in which helium is first absorbed on charcoal at various pressures and temperatures reached with conventional facilities and is subsequently evacuated, absorbing heat in the process, the temperature thus being lowered still further. The point of the process is that the helium absorbed on charcoal at various pressures effectively provides a series of substances with thermal properties varying continuously between those of hydrogen and helium. It is the lack of such substances in nature that requires complicated modifications of conventional low-tempera-



A photograph taken in 1929, showing Sir Francis Simon standing beside the apparatus with which he first studied the melting curve of helium up to a temperature of 42°K and a pressure of 5600 atmospheres. [Wide World Photos]

ture apparatus in taking the step from liquid hydrogen to liquid helium.

A few years after Simon suggested the "desorption" method, the "expansion" method was proposed. In this, helium is compressed to 100 to 200 atmospheres at the temperature of liquid hydrogen. Thermal contact with the hydrogen is then eliminated by pumping off an independent supply of gaseous helium, which had served as a thermal bridge between hydrogen and helium, whereupon the cooled, compressed, and thermally insulated helium is allowed to expand to a low pressure; during this time its temperature drops sufficiently that a very considerable fraction of the initial helium is liquefied—a fraction sufficient to permit the cooling of objects immersed in it and to make possible a wide variety of small-scale experiments. The method owes its feasibility to the reversal which takes place at low temperatures in the relative thermal capacity of the compressed helium and the solid parts of the containing vessels. Whereas at room temperature the thermal capacity of the metal vessels completely swamps that of the contained helium, at liquid hydrogen temperatures the thermal capacity of the metal has become vanishingly small as compared with that of the helium, so that we are effectively working with vessels with "mathematically thin" walls, as Simon liked to express it.

During this nine-year period Simon also directed determinations of the crystal structure of the solidified gases and of some other materials by x-rays, thus acquiring a first-hand acquaintance with x-ray techniques. However, he does not seem to have used these to any notable extent in his later years, although he did make use of the closely allied technique of the absorption of gamma rays in low-temperature calorimetry. Another activity which he did not pursue later was speculation about the electrical conductivity of metals in general. He proposed the rule that at equal reduced temperatures the electrical conductivity of metals with the same arrangement of electrons in the outer shells of the atoms is the same. He sought to obtain connections with the amplitude of atomic vibrations by utilizing the Grüneisen relation with atomic frequency. This occurred in 1924, a couple of years prior to the revolutionary new insight into the nature of metallic conduction afforded by Sommerfeld's application of the idea of quantum degeneracy in the electron gas of conduction electrons. Simon's line of attack was not greatly different from one which I myself was pursuing at the same time. After Sommerfeld's new insight, both Simon (to judge from his publications) and I abandoned concern with this problem.

The Melting Curve of Helium

Another line of Simon's interest, which continued throughout his life and which made pretty close connection with my own interests, began in the nine-year Berlin period. This was determination of the effect of pressure on the melting curve of many of the permanent gases. Data on this subject had already been obtained at Leyden, but the pressure range was only 100 or 200 atmospheres, barely enough to show the initial slope of the curve. Simon wanted to extend the pressure into the thousands of atmospheres; such pressures, he anticipated, would be high enough to have a significant bearing on the question of whether the melting curve ends in a critical point, as does the equilibrium curve between gas and liquid. Simon made a number of experimental attacks on this problem. Throughout, his experimental results were correlated with a semiempirical formula for the relation between temperature and pressure, which he published with Glatzel in 1929. This formula was suggested by a simple modification of the formula given by van der Waals' analysis for the vapor-pressure curve, the simple modification being to introduce the "internal" pressure of liquid or solid. This has the result of eliminating a negative infinity at zero pressure, a pressure which has physical meaning for a liquid or a solid but not for a gas. Besides work with this formula, in this nine-year period he carried the melting curve of helium to 42°K and 5600 atmospheres. The method was that of the blocked capillary. It is a disadvantage of the method that it permits a determination only of pressure and temperature, not of change of volume or latent heat. These parameters are related through Clapeyron's equation, and if they could be determined they would give some basis for extrapolating to higher pressures and temperatures and therefore some basis for judging whether a critical point is imminent, a judgment which is not possible in terms of pressure-temperature values only. To rectify this lack Simon also made some measurements of the latent heat of melting of helium up to nearly 2000 atmospheres and 20°K. The measurements were too rough to give any significant information. To make a direct determination of latent heat under high pressure is extraordinarily difficult and, in the ordinary range of temperature, is not feasible at all, because of the masking of the heat capacity of the melting substance by the heat capacity of the containing vessel. So far as I know, this measurement by Simon of a direct latent heat of melting under pressure is the only one of any value which has ever been made, except for a similar measurement made under his direction later. It does not now appear to offer a practical line of attack on the problem of the existence of a critical point, because with increasing pressure, temperature is rapidly carried to values so high that the heat capacity of the pressure vessel is of dominating importance.

Cooling by Expansion and Adiabatic Demagnetization

In 1931 Simon left Berlin, becoming *Ordinarius* (full professor) and director of the Institute for Physical Chemistry at the Technische Hochschule of Breslau, a post in which he was Eucken's successor. Late in 1931 his stay in Breslau was interrupted by a visit to the United States. He attended the AAAS meeting in New Orleans in December of 1931, where I had the pleasure of renewing my earlier acquaintance with him, and in the spring semester of 1932 he was visiting professor at the University of California at Berkeley. Even as early as this Simon was astute enough to forecast correctly the course of political events in Germany. The prospect alarmed him so much that he removed his family to Switzerland for the duration of his stay in the United States. On his return to Breslau he was instrumental in obtaining for me some special German steel for my experiments on high-pressure helium, without which the experiments could not have been successful. No steel at that time manufactured in this country was sufficiently free from mechanical imperfections to support high-pressure helium without leak. At Breslau Simon apparently had a good deal of administrative work; early in 1933 he wrote me that he had little time for scientific work because, in his capacity as dean of chemistry

and mining, he was actively engaged in the merging of the university and the Technische Hochschule.

It was during his stay in Berkeley that he developed his well-known expansion method for the liquefaction of small quantities of helium. He was able to demonstrate it as a lecture-table experiment and, with it, to demonstrate various phenomena connected with supraconductivity. The temperatures were indicated by his direct-reading gas thermometer. On his return to Breslau he set about converting the lecture-demonstration apparatus that he had used in Berkeley into a research tool for use at liquid helium temperatures, a tool particularly adapted for small laboratories. From Breslau during this period he published, with Kurti, a paper on the specific heat of gadolinium sulfate at low temperatures. His interest was connected with the then recently made proposal of Giauque and Debye for reaching low temperatures by the adiabatic demagnetization of paramagnetic substances. Kurti and Simon showed that there are low-temperature anomalies in the specific heat of gadolinium sulfate connected with the energy differences between the different orientations of the elementary magnets which would make the proposed method for attaining low temperatures less efficient than had appeared from the preliminary calculation of Giauque, but which nevertheless would not lessen the efficiency enough to prevent this from being a fruitful method for reaching temperatures hitherto unattainably low. However, this presumptive diminution of the effectiveness of the magnetic method caused Simon to turn to an examination of the possibility of reaching lower temperatures by another method, that of adiabatic decrease in volume. He proposed to use adiabatic decompression of liquid helium, carried out in a number of successive stages. Barring the appearance of unknown properties in the behavior of helium at unreached low temperatures, there appeared to be no theoretical limit to the temperatures which could be reached in this way. For this, Simon worked out an elaborate arrangement with metal bellows for the expansion engine. Since, however, the maximum theoretical gain for a single stage of the process was a lowering of temperature to one half the initial temperature, the practical application appeared too clumsy to make a serious working out of the idea worth while, and it appears to have been abandoned.

At Oxford University

The political situation continued to deteriorate, with Hitler in power, and by the summer of 1933 things had reached such a pass that Simon resigned his Breslau post and in September 1933 assumed the duties of a research position at Oxford—a post which he owed to the interest of F. A. Lindemann (later Lord Cherwell), head of the Clarendon Laboratory. I know from my own correspondence that Simon had been contemplating this step for some time and had made a pretty thorough canvass of the various possibilities, of which there were not many. His departure from Germany was greatly facilitated, as I know from personal communication, by the fact that he had been awarded the Iron Cross, first class. The help which he received was doubtless informal rather than official in character, the decoration being held in high esteem by all sorts of persons with whom he came in contact during this period. At Oxford his position did not at first amount to much more than a foothold in a new country, since the resources available there did not permit him to duplicate or even approach the facilities which he had left. He had only one room for his experimental work, and because of the dearth of technical and secretarial help he had to perform many routine tasks, including the typing of his own letters. His position gradually improved; in 1935 it was to a certain extent regularized by his succeeding A. C. G. Egerton (later Sir Alfred Egerton) as reader in thermodynamics, but expansion of the facilities continued to be slow. As late as 1936 Simon was not clear in his own mind whether, in justice to his own scientific career, he should regard his position as permanent. Matters gradually improved, however; a new laboratory presently came in prospect, and with the expanded facilities to be anticipated Simon could afford to bide his time a little longer. In 1939, just before the outbreak of the war, he acquired British citizenship, and in this he took extraordinary satisfaction.

The outbreak of World War II not only interrupted Simon's scientific work but upset his family life as well. In June 1940 his wife and two daughters were evacuated to Toronto, accepting the invitation of the faculty of Toronto University to care for the families of the Oxford faculty. Acceptance of this offer seemed essential to the Simons in view of the expected invasion of England, in which, it could be expected, they would be singled out for special treatment because of their German and Jewish origin. Simon stayed behind in England where for the first year of the war he experienced bitter frustration, since, in spite of his eagerness to make any technical contribution he could, his offers of help in the war effort were repulsed. This was due to the initial reluctance of the authorities to employ ex-enemy aliens on secret work. Debarred from "official" war work, Simon began to interest himself in the problem of the atomic bomb, in particular the large-scale separation of the uranium isotopes, well before investigation of these matters received government support. When, in the latter part of 1940, official work on the atomic bomb began, Simon became associated with it and was throughout the war one of the leading figures of the "Tube Alloys" project.

I am able to say very little about Simon's connection with the bomb and atomic energy. I had no personal connection with it, and in the nature of things his activities have not been published. It is well known that he was in charge of a major part of the isotope separation work of the British, and in connection with this work he visited this country a number of times during the war. His connection with the project, and in particular with the British establishment at Harwell, continued after the war on a part-time basis. The importance of his work was recognized, and in 1946 the order of Commander of the British Empire was bestowed upon him. In 1954 he was further rewarded, with knighthood. He took a frank and lively pleasure in these honors, and I have a vivid recollection of his acting out in front of my fireplace, with the fire tongs for a sword, the ceremony in which Queen Elizabeth dubbed him "Sir Francis." He at one time said of himself that he was doubtless the only person who had ever received an Iron Cross, first class, and also a CBE.

Immediately after the war he was made professor of thermodynamics, a new position created especially for him, and a Student of Christ Church—evidence of his full acceptance into the British academic community and tradition which he highly appreciated. From then on, with his new laboratory and adequate resources, his work rapidly expanded in importance and influence, until his laboratory at Oxford became one of the world's recog-

nized centers for low-temperature research. In 1956, on Lord Cherwell's retirement, he was made Dr. Lee's professor of experimental philosophy and head of the Clarendon Laboratory. He entered on his new duties only a few weeks before his death, on 31 October 1956.

Temperatures of a Few Hundredths of a Degree

We return now to Simon's scientific work at the time of his removal to Oxford in 1933. In spite of the meager facilities, a first paper appeared in *Nature* early in 1934, written jointly with Kurti, who had followed him from Breslau and remained associated with him at Oxford up to the time of his death. We have here a classical example of a fruitful scientific collaboration between two men, each complementing the other.

The subject of the first experimental paper with Kurti from Oxford was the production of very low temperatures by magnetic cooling, which, having been proposed in 1926 by Giauque and by Debye, was first carried out by Giauque in March 1933. As mentioned above, Simon had speculated a good deal on the experimental possibilities, and the fact that he had not attempted to test them earlier is evidence of his meticulous scientific courtesy and his unwillingness to trespass on a field opened up by another man before the latter had had opportunity to reap the first harvest. As a first result of reaching a temperature of 0.1°K, Simon and Kurti were able to announce a new supraconductor, cadmium. This experiment was made possible, as might be suspected, by Simon's expansion method for liquefying small amounts of helium. At about the same time Simon wrote a purely theoretical paper in which he showed that the unusual property of helium of remaining liquid to the lowest attainable temperatures and requiring the application of a pressure of 25 atmospheres to become solidified, was due to the unusually large role played by zero point energy, because of the weakness of the interatomic forces in helium.

One would never guess from Simon's bibliography after his transfer to Oxford that he was working with meager resources. In all, he published 49 scientific papers from Oxford between 1934 and 1942, when his scientific publication was suspended because of the war. His principal interest during the earlier part of this interval was development of the magnetic method of cooling. He did a number of experiments in Paris, using the magnet at Bellevue, at that time the largest magnet in the world. One of the most interesting of his discoveries was that of the ferromagnetism of a number of salts below their Curie points of a few hundredths of a degree—salts which above this point are paramagnetic. His work in Paris would hardly have been possible without his expansion method for working with small quantities of liquid helium.

An important problem in the newly accessible temperature region of a few hundredths of a degree is the measurement of temperature on the absolute scale, and Simon devoted considerable effort to this problem. Experimentally the simplest procedure, but a provisional one, on first entering this field, is to specify the temperature in terms of an assumed extrapolation of Curie's law that magnetic susceptibility is proportional to the reciprocal of the absolute temperature. However, this extrapolation is known to fail at low temperatures, as indeed it must, because if Curie's law continued to hold, absolute zero could be reached by an adiabatic demagnetization. The problem becomes, therefore, one of converting the apparent Curie temperatures into absolute temperatures. Simon did this by direct application of a Carnot cycle between the two temperatures, using the fundamental formula of the second law:

$$Q_1/T_1=Q_2/T_2.$$

Here T_2 is the unknown temperature in the new region (defined and reproducible, however, in terms of its "Curie" temperature), and T_1 is a temperature in the region of higher temperatures in which the absolute scale has already been established. Application of the second law and determination of the absolute temperature T_2 therefore is reduced to a determination of the quantities of heat Q_1 and Q_2. Q_1 may be assumed to be measurable by methods already in our command, but the measurement of Q_2 in the new region is not simple. Simon solved the problem of measuring Q_2 by absorbing a known quantity of gamma rays, a most ingenious method which he utilized on a number of other occasions, and which he subjected to a careful analysis to be sure that there were no flaws in it.

In connection with this question of reduction to the absolute scale of temperature, Kurti has called it to my attention that Simon in two of his earliest papers effectively established absolute temperatures in the hydrogen region by a direct application of the second law of thermodynamics to the sublimation equilibrium of solid hydrogen at different temperatures, using the triple point as the fiducial point at which the absolute temperature is assumed known. Such a direct thermodynamic establishment of an absolute temperature is frequently discussed in the textbooks but is practically never carried out.

Liquid Helium II

The properties of liquid helium was another subject actively pursued by Simon during the prewar years in Oxford. An important procedure in this work was the first explanation (with Rollin) of some of the very puzzling phenomena shown by helium II as being due to a thin creeping film which covered all the accessible surfaces of the containing vessel.

Nernst's Heat Theorem

Simon spent the first couple of years after his return to Oxford after the war in equipping the new laboratory. Novel apparatus and methods were used which were the subject of numerous papers. His first paper of the postwar period was not published until 1948, six years after publication of his last previous paper. This interval of six years is, by coincidence, exactly the same as the time lost from his scientific career because of military service during World War I.

In his postwar period Simon returned to re-examining and making more precise his earlier work with Nernst's heat theorem. To this he devoted several papers, of which by far the most important is the Guthrie lecture to the Physical Society, delivered in March 1956. In this lecture a detailed history is given of the various developments, and the magnitude of Simon's own contributions appears particularly clearly. Simon was qualified as no other man to give this summary, both because of his early association with Nernst and because of his own contributions. Simon's contention in his lecture was that Nernst's heat theorem has by now, by general recognition, become truly a

third law of thermodynamics, coordinate in importance and range with the first and the second. A great deal of the early skepticism about the law was based on the many anomalies in specific heats at low temperatures. It was largely due to Simon's own work that nearly all these anomalies were recognized to be quantum effects which result in abnormal and temporary liberations of energy during the degeneration of various degrees of internal freedom, followed at lower temperatures by domains in which the complete order is attained, a situation which means zero entropy for all the regular phases.

In addition to his work on the many substances with recognizable quantum anomalies, Simon was probably the first to recognize and insist on finding an explanation for another class of apparent exceptions to the third law, exemplified by the glasses and subcooled solutions. Here we are dealing with "frozen-in" degrees of freedom, by virtue of which the substance is incapable of coming to a state of true thermodynamic "equilibrium." According to Simon, such systems are not properly thermodynamic systems at all, and it is not to be wondered at that the third law of thermodynamics does not apply to them.

In arriving at this position Simon was led to examine with great care the concept of a "glass," and he made the very important factual discovery that in the comparatively narrow temperature interval within which the degrees of freedom are in process of being frozen in, other characteristic phenomena occur, such as abnormal behavior of the specific heats or of the temperature-dependence of viscosity. It seems to me that it is here, in his factual discoveries, that Simon's important contribution is to be found, rather than in his insistence that we have here a true third law of thermodynamics, which appears to me to be to a certain extent a verbal matter. For after all, conventional thermodynamics does control the ordinary behavior of glasses; no one would hesitate to apply to them such a formula as, for example,

$$\left(\frac{\partial C_p}{\partial p}\right)_T = -T\left(\frac{\partial^2 v}{\partial T^2}\right)_p$$

which is derived from the first and second laws.

Another topic which occupied Simon in his postwar period was the nature of the melting curve. He was never satisfied that my own experiments to high pressures on various melting curves had sufficiently established the improbability of a critical point between liquid and solid, and he always ascribed a particular significance in the answering of this question to a determination of the melting curve of helium to the highest feasible pressures. Already, before leaving Germany, he had, in collaboration with Ruhemann and Edwards, followed the melting curve of helium to 5600 atmospheres. With an altered technique he was now able, in collaboration mainly with G. O. Jones and D. W. Robinson, to carry the melting curve to nearly 10,000 atmospheres and 60°K. It was still a disadvantage of his method that he was not able to determine all the parameters necessary to completely characterize the transition thermodynamically. This effect was now rectified by new work at Oxford by Dugdale, who was able to measure directly the specific heat of compressed solid helium and the heat of melting up to 3000 atmospheres. It appeared from these thermal measurements that the entropy difference between liquid and solid increases along the melting curve with increasing pressure and temperature. Since at a critical point the entropy difference vanishes, here is strong presumptive evidence that the melting curve of helium, at least, is not headed for a critical point.

Simon closely correlated his experimental work on this topic with the formula which he had derived as early as 1929 on a semiempirical basis, namely,

$$\log\,(p + a) = c \log T + b.$$

At that time a connection was indicated, by analogy, between the constants of the formula and the internal pressure of van der Waals. On Simon's return to the problem after the war, various collaborators succeeded in strengthening the theoretical implications of the formula, in particular Salter, by showing a connection with the internal pressure (of the gas). Now the experiments on helium are unique in the enormous range of both pressure and temperature when expressed, as reduced parameters, in terms of the critical constants of vapor-liquid. The attainable temperatures and pressures for helium are of the order of ten times the critical temperature and 500 times the critical pressure, whereas for many ordinary substances these factors may be of the order of unity and 100, respectively. Because of this Simon felt justified in applying the same type of formula by extrapolation to other substances, and in particular to the melting of iron. This formula predicts, for the pressure at the interior of the earth, 4×10^6 atmospheres and a melting temperature of the order of 4000°K, a result of obvious interest to the geophysicist.

Diamond-Graphite Transition

A closely related topic which received Simon's attention after the war was the diamond-graphite transition and equilibrium. The calculation of the location of the transition line afforded a natural application of the third law, with some unusual features because of the abnormally low specific heat of diamond at room temperature. As early as 1926 Simon had published an estimate of the equilibrium relation between pressure and temperature. After the war he returned to the question and with Berman published a paper in which the most recent thermodynamic data were used, including certain new data obtained by other collaborators at Oxford, in order to rule out the possibility of unsuspected low-temperature anomalies. This calculation of the probable course of the equilibrium was unique in its careful estimate of the uncertainty introduced by various doubtful factors. Simon and Berman calculated an equilibrium pressure at 1200°K of approximately 40,000 kg/cm², with a linear rise above that at the rate of 27 kg/cm² per degree. Before the paper appeared in print, the General Electric Company had announced the successful synthesis of diamond. The details have not yet been published, but from everything that has appeared it would seem that the General Electric conditions fall within the limits of Simon and Berman's estimates. Simon also had a novel idea about the synthesis of diamond. His idea was that the great reluctance of graphite to take up the stable diamond form at low temperatures and high pressures could be overcome by neutron bombardment, and he had some calculations to justify this. He constructed an apparatus in which an ostensible pressure of nearly 30,000 atmospheres could be imposed on a piece of graphite of sufficiently small dimensions; the graphite was then subjected to prolonged neutron bombardment in the pile at Harwell. The results were negative. Simon believed that the apparatus actually did not deliver the required pressures and expected to repeat the experiment under more favorable conditions.

Thermal Conductivity

Another area of postwar activity for Simon was thermal conductivity, particularly of dielectric crystals. The measurements demanded control of temperature over the entire range, from room temperature down, and, as such, demanded certain changes in his customary technique. Working with Berman and Wilks he clearly demonstrated the two fundamental processes responsible for thermal resistance in dielectric crystals: the Umklapp type of process between phonons, first proposed by Peierls, and scattering at the boundaries, proposed by Casimir. These experiments were subsequently extended in considerable detail and illustrated the influence of lattice defects.

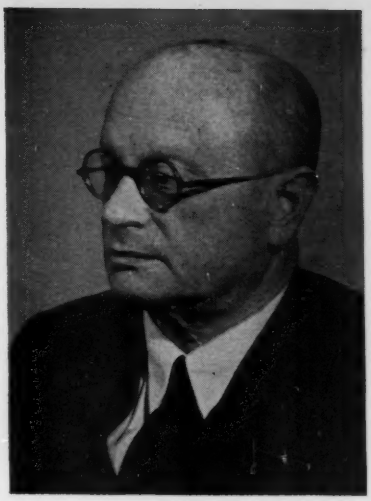
Nuclear Orientation and Cooling

Without doubt the most important part of Simon's postwar scientific work was that connected with nuclear orientation and, in particular, with nuclear cooling, in which the magnetic moments of the nuclei of the atoms at excessively low temperatures are utilized in very much the same way as the paramagnetic moments at higher temperatures. This possibility was suggested soon after the paramagnetic method had been proposed, by Gorter in 1934 and nearly simultaneously by Simon and Kurti in 1935. There were, however, formidable technical difficulties when it came to putting the idea into practice. The initial temperature from which the demagnetization was to occur had to be of the order of 0.01°K. Heat transfer between the magnetic and the nonmagnetic parts of the system also introduced complications. After long preparation, successful results were obtained by Simon, with Kurti, Robinson, and Spohr, and the announcement that temperatures of about 2×10^{-6} °K had been attained was published in *Nature* only a few weeks before Simon's death. Thus, a new field was opened for understanding the properties of matter.

Public Figure and Humanitarian

Simon's connection with the atomic energy project both during and after the war brought him into such close contact with industry and public affairs that it was only natural that concern with broad questions of public policy should occupy a large part of his time after the war, and he became increasingly recognized as a public figure. His usefulness in the field of public policy was increased by his extensive travels, which had given him a wide first-hand acquaintance with conditions in Europe and America. At the invitation of the *Financial Times* of London he wrote an extensive series of articles, which received wide attention and doubtless exerted considerable influence. In these articles he emphasized especially the importance for England of an adequate supply of scientists and engineers, he called attention to the woeful lack of educational facilities, and he strongly urged the immediate establishment of several technical institutes of the caliber of the Technische Hochschule in Zürich or Massachusetts Institute of Technology. He saw the widespread ignorance of scientific matters, on the part not only of the lay public but also of the members of the boards of directors of many large British industries, as part of the same picture and as the cause of the country's having fallen far behind in the technological race with other countries. A number of his articles on this theme were collected into a little book, *The Neglect of Science*. He was vividly aware that a crisis was approaching in the country's power supply, chiefly in the supply of coal, which was continually becoming less adequate. He called attention to very wasteful practices in the regular use of coal. He put his finger on the single most important factor in this waste—the almost universal habit of heating houses with open coal grates. More efficient methods of house heating would, he calculated, save 20 million tons of coal a year, a vital amount in the present state of the economy. As a simple method of eliminating this waste he suggested replacing the open grates with closed stoves, and he urged that the government distribute such stoves free to all who would use them. Many people recognized the justice of his comments, but it does not appear that anyone has done anything about it.

In connection with the fuel-economy picture, Simon was much concerned with the development of atomic power for industrial purposes, and he wrote a number of articles on the practical aspects of the utilization of atomic energy and the probable timetable for such utilization. He was concerned lest people should be too optimistic and think that we have here an easy solution of all our troubles. On his return from the Geneva conference of 1955 on the peaceful uses of atomic energy he wrote me that he was disturbed because he felt the small nations had been given an all-too-rosy picture of the prospects for them.



Sir Francis Simon

Kurti has remarked that there seems to be a single guiding motif running through all Simon's concern with public affairs—namely, his horror of waste in any form, fostered by his life-long experience with the third law of thermodynamics and his vivid appreciation of the significance of the growth of entropy. When, at the end of the war, he abruptly stopped smoking, he remarked, perhaps jocosely, that for the money that went up in smoke he had better help someone in need.

It remains only to pay a tribute, however inadequate, to his personal qualities, through which he endeared himself to all with whom he came in close contact. Perhaps his most outstanding trait was his intense interest in people for their own sake. His interest manifested itself in many practical works, which ranged from helping a student find a boarding place for himself or his family to elaborate efforts to get refugees out of Nazi Germany, which resulted quite literally in the saving of lives. His home in Oxford became a sort of clearinghouse for scientists who had to leave Germany and for whom he tried to find positions. As a result of his own experience in getting out of Germany and of his later experience in evacuating refugees, he acquired a certain know-how, not without an element of cynicism, for dealing effectively with officialdom. At the time of his visit to this country in 1953 to attend the Rum-

ford celebration of the American Academy of Arts and Sciences I happened to be sufficiently behind the scenes to witness Simon's tactics to ensure that he did not encounter the inconveniences in getting an American visa that were being experienced by so many other foreign scientists at that time; these tactics can only be described as masterly!

The rapid turnover of his secretaries was legendary, it being the almost invariable rule that after a year or two they left to marry a graduate student or one of his young colleagues—something which gave Simon the keenest pleasure in spite of the obvious inconvenience to him.

He had a refreshing and somewhat Puckish wit, of which the subject might equally well be himself or someone else. He had his foibles; his dislike of drafts and of the insufficiently heated English rooms (conditions which caused him to don, as occasion demanded, cap and sweater and muffler) is well known. Although he flew a great deal, he did not like flying, and before pressurized cabins were common he always traveled with an oxygen bottle, apparently for very real reasons of health. However, his colleagues were not always sympathetic about such measures, and on at least one occasion Simon's maneuvers to supply himself with his oxygen flask without public knowledge provided much entertainment to those who knew of them.

Simon's adaptability in transplanting himself to a foreign country at the age of 40 and making himself so valuable as to be officially rewarded in the way he was is well-nigh unique in these times. He felt himself thoroughly at home in England, although he continued to hold the view that in some respects the English were a little mad. He even acquired some of the exterior markings of an Englishman. I remember that after he had appeared in the dining-room of the local hotel, in the New Hampshire town where I have my country home, one of my friends, on learning that it was *Sir* Francis Simon, remarked, "I spotted him at once for a typical Britisher." I am told, however, that his British colleagues were under no such illusions. His English was fluent, but he never felt that he had mastered all the niceties of the language, and he always gave his papers to friends for editing before submitting them for publication.

To a high degree his was a vital, almost exuberant, personality. To it was due in great measure the richness and fertility of his scientific ideas, which permitted him to catalyze so successfully the work of his many students and colleagues. It is hard to realize that we shall no more see him in our midst, and hard to justly assess the magnitude of our loss. His memory will long be with us.

Note

In preparing this article I received valuable assistance from a number of Simon's intimate associates, in particular N. Kurti and Lady Simon.

Charles Judson Herrick, Neurologist

The leitmotiv of Charles Judson Herrick's scientific career was the integration of the several disciplines involved in the understanding of the nervous system. When such a plan was originally envisioned by his elder brother Clarence, in 1891, the factual basis was meager, and a frontal assault on the barriers between university departments could not be tolerated. In the course of 70 years, "C.J.," as his friends called him, contributed a wealth of histological detail on the fundamental structure of the nervous system and correlated his findings with the rapid advances of the 20th century in psychology, physiology, and psychiatry.

From early boyhood his brother had been his teacher in "natural history," and before graduating from college Herrick began to study the brains of fishes. In 1893 his brother had to resign his professorship at Denison University and hasten to Arizona because of tuberculosis. "C.J.," two years out of college, undertook to conduct, single-handed, all of the scheduled courses in biology. He assumed an even heavier burden in saving the *Journal of Comparative Neurology*, which his brother had founded, "because its suspension would break my brother's heart and retard his recovery." He became editor, business manager without a secretary, proofreader, and supervisor of the printing of engravings. Until 1908 he paid the inevitable deficits of the journal from his own salary. On one occasion he felt called upon to decipher a manuscript in longhand which had been charred black in the fire which destroyed his laboratory. Many a robust man would have been overwhelmed by these labors. They did not keep C. J. Herrick from continuing his research.

After a year's leave of absence in 1896 he completed his doctoral dissertation, a monograph of 302 pages. It was the first complete analysis of the cranial nerves and contributed more than any other single work to the establishment of the "American School," which was interpreting the structure of the nervous system in terms of its functions. By 1907 he had published 17 papers on the nervous system of fishes in which certain afferent systems are highly developed and their centers in the brain are hypertrophied, so that the connecting fibers can be followed without experimental interference. A study of the behavior of normal catfish (1902) clearly demonstrated the gustatory function of the "terminal buds" which are distributed over the skin of head and trunk.

The reputation established by these studies resulted in an invitation to Herrick to become professor of neurology at the University of Chicago when H. H. Donaldson resigned to go to the Wistar Institute in 1907. Herrick hesi-

tated to make so radical a change because of his health. The matter was decided by his wife's question, "Would you rather go to Chicago and burn out or stay here and rust out?" He joined the department of R. R. Bensley, who was in the midst of correlating the structural changes in cells with the cell functions. The change provided ample facilities, technical help, and leisure for research, gave him inspiring colleagues in the university, and freed him from executive duties. A year later he presented the *Journal of Comparative Neurology*, free of debt, to the Wistar Institute, so that he was relieved of much of the labor it involved. He continued to guide its editorial policies until 1948 and saw it become one of the outstanding biological journals.

His training and his extraordinary patience and persistence led him to continue the histologic analysis of the brain as a working mechanism. He turned from the highly specialized fishes to the primitive and generalized tailed amphibia. In the course of 40 years he made the most complete analysis that we have of a vertebrate brain. It culminated in publication of *The Brain of the Tiger Salamander* (1948). He showed that the larval and adult nervous systems are primitively organized for responses of the body as a whole to normal environmental stimuli. This mechanism for the stereotyped, inherited behavior has evolved in all species that have survived through the ages. All living creatures are also capable, to a greater or lesser extent, of adaptation to change. The elaboration of the ability of the individual to learn by experience has, in vertebrates, gone hand in hand with the development of the cerebral cortex. In the light of his studies of reptiles and primitive mammals, Herrick could recognize in the amphibian forebrain the primordia from which the cortex has evolved. This provided the fundamental plan for the entire vertebrate brain and has been invaluable for the analysis of more complex brains. These topics were elaborated in his *Neurological Foundations of Animal Behavior* (1924). The relations of brain and mind were clarified for "the man in the street," as he used to call him, in *The Brains of Rats and Men* (1926). He wrote *The Thinking Machine* (1929) with the needs of students of psychology in mind.

Charles J. Herrick

Teaching was an essential part of his mission. During the first year at Chicago he organized a new type of course in neuroanatomy and gave a brilliant series of lectures on the evolution of the nervous system and its functions to the group of advanced students his coming had attracted to the university. To those of us who had encountered only the current textbooks of anatomy, he opened up a new and fascinating field of study. His "seminar in neurology" was popular, and as members of other departments were invited to contribute, it developed into the stimulating Neurology Club of the University.

In 1914 there was still no elementary textbook on the nervous system, so he wrote one. Its unique point of view soon stimulated the production of a whole flock of texts designed for the courses in neuroanatomy patterned after his. His *Introduction to Neurology* provided a background for his course for psychology students, offered from 1916 to 1934 (except during his service in the Army). This was a true university course, without formal lectures or examinations and with unlimited opportunity for discussion. J. B. Obenchain assisted and later continued it. From the outset scholars from abroad as well as many advanced students came to work in Herrick's laboratory. To them his splendid library as well as his advice was always available.

Ten years after his retirement to Grand Rapids, Michigan, he found it necessary to give up the long hours of observation at the microscope. He devoted himself to meditation, reading, and writing on the philosophy of science. In his last book, *The Evolution of Human Nature* (1956), he brought sociology into the field of psychobiology. He died at the age of 91 on 29 January 1960, before he had completed all he had planned to do.

GEORGE W. BARTELMEZ
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Science in the News

Atomic Radiation Hazards: Congressional Hearings on Radiation Standards

For the past two weeks the Joint Congressional Atomic Energy Committee has been holding hearings on the touchy and difficult problems connected with the health hazards of atomic radiation.

The committee has held similar hearings on half a dozen occasions in recent years, covering fallout, radiation waste disposal, and similar topics. As in the case of the present inquiry, a principal purpose has been to compile a detailed record, in comparatively nontechnical language, of the thinking and the results obtained by several dozen of the leading scientists professionally engaged in work with radiation problems.

The committee has not attempted to minimize the dangers of radiation. In fact Congressman Chet Holifield (D-Calif.), who has been chairman of the hearings, has on several occasions used leading questions to get witnesses to amplify remarks which, if taken out of context, might seem to minimize the dangers. Nevertheless, the committee and the expert witnesses it has called to testify are pretty clearly concerned about the sometimes exaggerated public reaction to the whole business.

Public Reaction

The reaction is understandable; it stems from the nature of the problem, which is, to the layman, concerned with things mysterious, invisible, largely unavoidable, and associated at their source with atomic weapons and in their results with such things as leukemia, bone cancer, and genetic mutations. What the committee is trying to do, aside from keeping itself informed on a subject which is part of its legislative responsibility, is to allay excessive fears by making detailed information available to the public. The committee hopes that this will at least help put the problem in proper perspective.

The level of average radiation exposure today, the committee was told, is estimated to be about equal to the natural background radiation with which man has always lived. The witnesses made it clear that the best figures are only estimates, but that nevertheless the fact that a number of independent U.N., American, and British groups have all arrived at substantially the same figures makes them seem quite reliable.

A study prepared for the National Academy of Sciences, and these figures are in close agreement with those reached in other studies, indicates that, taking the level of natural radiation as 1, the level of man-made radiation would be about 0.9. Most of this (85 percent) stems from medical and dental diagnostic x-rays, and an additional 8 percent from medical x-ray therapy. Nothing else seems to amount to more than 1 percent of the total man-made radiation. Three of these minor sources are of special interest to the public: internal isotopes (mostly strontium-90 fallout plus certain other isotopes which tend to be assimilated into the body), incidental radiation from television sets, and luminous dials (mainly on wristwatches). Each of these sources amounts to about 1 percent of the man-made total.

The fallout figure, of course, is based on present contamination. If large-scale weapon testing in the atmosphere were to be resumed it would increase substantially. But at this time neither the U.S. nor Russia has shown the slightest indication of resuming such testing. The negotiations at Geneva are concerned with underground and outer space testing, neither of which produces any fallout at all.

How dangerous is the present level of radiation? No one knows the answer to this, although this fact, in itself, suggests that the danger may be small compared to, let us say, the danger of being killed in an auto accident. At this time, the committee was told, a clear cause and effect relationship between radiation exposure and body damage has been established only for large doses of the order of many hundreds and more times background radiation.

Even here most of the data are based on cases where the doses were received in an "acute" form—that is, in the form of a massive dose delivered in a short period of time. At lower rates, but still much above the average population level, there begins to be some statistical evidence linking, for example, an increase in leukemia incidence to the profession of radiology, where the practice of the profession necessarily leads to chronic exposure to levels much above the average. Studies in this area, the committee was told, while sometimes inconclusive, tend to show that radiologists have a slightly lower life expectancy than medical specialists in general, but still slightly higher than general practitioners. Getting down to normal levels, there is not even any significant amount of statistical evidence suggesting damage. The witnesses before the committee nevertheless felt it had to be assumed, for safety's sake, that such marginal radiation is harmful. But apparently the level of harmfulness at anything near present levels of average exposure, if it exists at all, is low enough so that it takes careful statistical analyses of large numbers of cases to discern any effect.

Problem of Statistics

The problem of statistics raises several questions close to the heart of the over-all problem. In the first place the available studies give information mainly on adults, and there is some reason to believe that these data may not be strictly applicable to children, and stronger evidence that they are not easily applicable to unborn children, particularly in the first few months after conception. Fallout tends to be disproportionately high in certain latitudes and its effects still more disproportionate under certain soil conditions and where the prevailing diet is weighted toward dairy products. The major figures of average exposure to medical and dental uses of x-rays are obviously not distributed evenly among the population. All the x-ray therapy is done on a rather small group of people, and even the general diagnostic x-rays vary widely in the incidence on different individuals.

None of this unevenness of distribution invalidates the consensus of the

testimony summarized here. They are taken into account in the detailed studies that are being made. But they do point up the complexity of the subject and show how statistics carelessly used can be thoroughly misleading.

Individuals versus Averages

Even if one could assume homogeneity of distribution, there was still a great deal of discussion before the committee over how one ought to regard such over-all statistics. There was wide agreement among the witnesses that, measured in terms of averages, anything close to present exposure levels is probably quite insignificant—a matter of lowering average life expectancy by a day or two. This unquestionably is more than balanced by the increase in life expectancy stemming from the use of medical x-rays and other types of useful radiation. But measured in terms of individuals the figures are much less easy to ignore. The committee heard estimates, for example, that an extra 2000 leukemia deaths are likely to result from present levels of radiation. Looked at in terms of such absolute rather than average figures it becomes clear that any step that reduces the amount of radiation exposure is likely to save lives even though no one will ever be able to point to any specific individual and say that "his life was saved."

The dilemma the scientists face is how to set standards in this field. It is generally accepted, though not proven, that there is no dosage level below which radiation becomes harmless. It is assumed that any increase in radiation, whether natural or man-made, will carry with it some increase in damage, even though this damage may not be detectable even in the most elaborate statistical studies. Even where the damage is fairly calculable (by assuming linearity and the nonexistence of a threshold as was done with the leukemia estimates given above), there is no special point at which it suddenly becomes clear that drastic measures are called for to prevent any further increase. Roughly 40 thousand Americans die in auto accidents every year, a fact which leads no one to recommend that automobiles be outlawed. What is done is to formulate standards for drivers, for highways, and for the cars themselves to keep the level down to a point which society seems to be willing to accept and, further, to minimize the death rate below the acceptable level.

In the case of radiation analogous reasoning has led to two general principles: first to try to set standards, necessarily arbitrary, of acceptable degrees of risk in various situations; and second to look for all reasonable ways to minimize radiation even for cases which are well below acceptable level in order to minimize damage within this acceptable range. Given the lack of precise knowledge, these standards, for the population at large, tend to be set in terms of natural radiation. Virtually everyone who has studied the situation agrees to this principle, the reasoning being that, since man has always lived with natural background radiation without any disastrous effects, a level of man-made radiation of the same order of magnitude should be relatively acceptable. And although this assumption is unproved, and presumably will remain so for several generations, it does, as noted earlier, tend to be confirmed by the limited statistical and clinical data now available. In line with this reasoning, the current standards set by various groups range from 1 to 2 times the background radiation as an *average* for the population at large. It is assumed that this will not lead to the exposure of large numbers of individuals to more than 5 times this level and this, it is believed, still leaves a substantial margin of acceptable risk.

Actually these figures simplify the situation considerably. They give an accurate enough picture for the general reader. But in fact, an extremely complex variety of standards have actually been set, specifying guidance levels for various situations, types of hazards, and for exposure of different parts of the body. Substantially higher levels have been set for special occupational groups, in the same way that society allows miners, chemical workers, construction workers, and other groups to accept hazards which would not be acceptable for the population at large. Many of these standards are specified in recent publications of the Federal Radiation Council, which is chaired by Arthur Flemming, the Secretary of Health, Education, and Welfare. The committee has heard detailed presentations of what is being done to see that these standards, once set, are enforced. A report of the problems of enforcement will appear here next week.

Aid to Education: Bill Passes House, But Outcome Still in Doubt

The House of Representatives climaxed a 10-year effort to pass a federal aid to school construction bill last week, but it was difficult to tell who had actually won the battle. There is considerable doubt that the bill will ever reach the President's desk, and more doubt about whether, if it reaches his desk, it will be in a form he will be willing to sign.

The bill provides for $325 million a year in federal aid for each of four years, with the states and localities required to match the government grants. The grants would be prorated among the states on the basis of numbers of school age children. They would provide enough money to build about 50,000 classrooms; the officially estimated shortage is 132,000 classrooms. In the Senate, a much broader billion-dollar-a-year bill was passed in February.

Those who are leery of federal aid, as is the President, tend to be at best passively in favor of a modest bill granting emergency assistance primarily to the neediest states. More commonly they are actively opposed to any bill on the grounds that it will be the opening wedge for a much mo e massive program in the future. In this contention they are almost certainly correct since a large, continuing program is exactly what most of those in favor of federal aid feel is needed.

Powell Amendment

Supporters had hoped that the House bill, a compromise which they felt the President might sign, would be accepted by the Senate, thus avoiding the need for sending the bill to conference. This possibility was eliminated when the Powell amendment was attached. The amendment, heavily backed by House Republicans opposed to any federal aid bill, provides that the grants should not be used to build segregated schools. No Southerner who would like to be re-elected could vote for a bill containing this provision, which means that such a bill cannot get through the Senate, where a filibuster can be used to prevent a vote.

This means that the bill will have to go back to the House Rules Committee in order to get to conference, and the majority of the Rules Committee is opposed to federal aid.

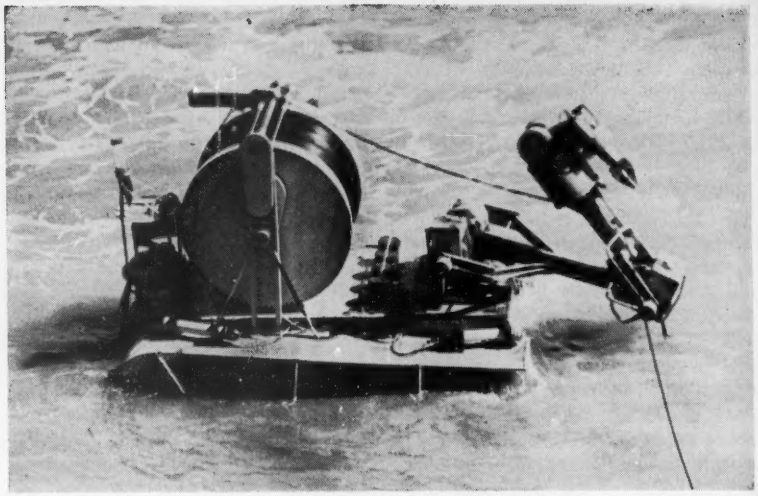
Remote-Control Undersea Vehicle Demonstrated

A remote-control undersea vehicle for conducting scientific studies of the ocean bottom for prolonged periods at great depths has been developed for the Office of Naval Research. The new vehicle, called RUM for Remote Underwater Manipulator, was demonstrated off the shore of La Jolla, Calif., on 16 May. RUM is essentially a tank equipped with a long, jointed manipulator arm and hand, together with specially devised underwater television cameras which serve as the eyes of the vehicle's operator on shore.

The development of the vehicle was directed by Victor Anderson of the University of California's Scripps Institution of Oceanography, La Jolla, for use in cooperation with the Hudson Laboratories of Columbia University. The goal of the RUM program is to develop an efficient oceanographic vehicle for use in observing the sea floor, collecting samples and specimens, and assembling and installing deep, bottom-mounted instrumentation.

The experimental vehicle that was demonstrated at La Jolla can operate at depths down to 20,000 feet, maintaining a speed of 3 miles per hour where the ocean floor is level. It can maneuver and operate on a 60-percent incline and is capable of climbing a vertical obstacle 12 inches in height.

RUM is linked to a mobile van by a coaxial cable long enough to permit operations out to 5 miles from shore. The cable carries the television signal, relays power for the operation of the vehicle and its cameras and mercury-vapor lights, and provides several remote-control telemetering channels.



New remote-control underwater vehicle (RUM) developed for the Navy by the Scripps Institution of Oceanography.

International Space Academy Established by Guggenheim Grant

Formation of an International Academy of Astronautics to provide world technical leadership for the peaceful conquest of space and to serve as a clearing house for astronautic information has been announced by the International Astronautical Federation and the Daniel and Florence Guggenheim Foundation, New York. The academy, which will be financed for the first 3 years with $75,000 from the Guggenheim Foundation, will be composed of leading scientists in the basic sciences, engineering, the life sciences, and other major fields involved in the development of astronautics. It will be a part of the federation, whose president is Leonid I. Sedov of the U.S.S.R. Academy of Sciences.

Theodore von Karman of the United States, chairman of the founding committee that was authorized to establish the academy at last year's London meeting of the federation, has commented that the new association will provide "what may be the only common intellectual meeting ground" for the scientists and engineers of all nations, including those of both East and West. Consequently, he said, the academy can "bring about the peaceful exploration of space in the shortest possible time, and for the greatest benefit for all."

Sedov said in Moscow that the foundation's grant would be reported at the forthcoming Astronautics Congress in Stockholm and would be "highly appreciated and used for furthering international cooperation and successes in astronautics."

The International Astronautical Federation has been functioning since 1950 and consists of representatives and delegates of the rocket and astronautical societies of some 30 countries. The United States member, the American Rocket Society, has more than 15,000 members. Apart from the federation, there are only two major international groups in the field. These are the United Nations Committee on Peaceful Uses of Outer Space and the Committee on Space Research of the International Council of Scientific Unions.

The Academy's Functions

The principal powers and functions of the academy will be to:

Provide advice to the president of the International Astronautical Federation when requested.

Hold scientific meetings and make scientific studies and reports.

Publish *Acta Astronautica*, an international technical publication devoted to astronautics.

Award medals and prizes intended to further progress in the field of astronautics, and carry out such other tasks as may be considered desirable for promoting the advancement of astronautics.

Present plans call for the establishment of the academy as a going concern at the Stockholm meeting in August. It is expected that a permanent headquarters will be established in Paris, where the French Government has promised rent-free quarters.

Federal Regulation of Animal Studies Proposed by 11 Senators

A bill to regulate the use of experimental animals by institutions receiving federal grants or working on federal contracts was introduced on 18 May

by Senator John Sherman Cooper of Kentucky. The bill, S. 3570, is cosponsored by Senators E. L. Bartlett and Ernest Gruening of Alaska, Robert C. Byrd and Jennings Randolph of West Virginia, Estes Kefauver of Tennessee, Mike Mansfield of Montana, Pat McNamara of Michigan, Wayne Morse of Oregon, William Proxmire of Wisconsin, and Joseph Clark of Pennsylvania.

Lester P. Dragstedt, president of the National Society for Medical Research, although strongly opposed to the bill, hopes that it will receive a thorough hearing so that members of Congress will develop a greater interest in promoting funds for laboratory animal care. Dragstedt comments that the proposed new law appears to be "an attempt by the anti-vivisection cult to strangle medical research with red tape." He points out that the bill has no constructive provisions and "makes no provision for helping scientists attain better methods, better helpers or better facilities."

The National Society for Medical Research is comprised of more than 500 national scientific and health organizations. Its members include every medical college in the United States and most of the colleges of dentistry, pharmacy, and veterinary medicine. The society also represents the larger clinics, hospitals, and institutes that are engaged in research in addition to patient-care activities.

The Bill's Provisions

The Cooper bill would license every individual scientist who might work with animals under any federal grant or contract.

Laboratories in which animal research were to be conducted would be inspected and issued "certificates of compliance."

Research plans would have to be submitted to the Secretary of Health, Education, and Welfare for approval, and presumably there would be no exceptions.

Detailed annual reports would have to be made on each licensee, covering all experiments performed during the year.

The National Institutes of Health would be assigned police duties.

The Cooper bill is identical in its major provisions to the German law adopted when the Nazis first came to power in 1933. The law did not prohibit animal experiments, but it so encumbered animal experimentation that it was cited at the Nuremberg trials as one reason why some Nazi experimenters turned to the use of prisoners in concentration camps.

The Cooper bill is also similar to the British law of 1876, except that, unlike the British law, it does not require surgeons to perform their first operations on human patients.

U.S. Launches 5000-Pound Missile-Warning Satellite

A 5000-pound United States satellite was placed in orbit by the Air Force on 24 May. The 22- by 5-foot vehicle was launched from Cape Canaveral, Fla., as part of a program to develop a missile attack warning system. Called Midas, for Missile Defense Alarm System, the new satellite is circling the earth about every 94 minutes at a distance of approximately 300 miles. Its inclination to the equator is 28 degrees.

The Midas is not expected to operate as a reliable detection device until 1963. The perfected Midas would be capable of detecting the heat radiation from the exhaust flames of a missile's rocket engine as the missile left the atmosphere; then infrared "feelers" would immediately relay a warning.

The Air Force estimated that the new vehicle, which carries 3000 pounds of instrumentation, would have a useful life of 3 to 4 weeks before its battery power is exhausted. However, on 26 May it was reported that something had gone wrong with the system for radioing orders to the satellite. This spoiled plans to test the vehicle's detection instruments during a missile launching and during the firing of a series of flares from Edwards Air Force Base. Midas is expected to remain in orbit for 3 years.

AAAS Group Studies Use of Special Teachers

A conference on the use of special teachers of science and mathematics in the elementary school was held at the Burlington Hotel in Washington, 15–16 May. The conference, sponsored by the Science Teaching Improvement Program of the AAAS, was attended by about 75 persons from some 25 states. Among the participants were scientists, including directors of the National Science Foundation curriculum studies; school superintendents; and elementary-school principals, supervisors, and teachers.

The program included a report of the STIP Study on the Use of Special Teachers in Grades 5 and 6, which is being conducted with the cooperation of the school systems in Cedar Rapids, Iowa; Lansing, Mich.; Washington, D.C.; and Woodford County, Ky. Equal attention was given to the Dual Progress Plan, which is New York University's experimental study of the use of special teachers in the elementary school.

The purpose of the conference was to determine what has been learned during the past year from these two experimental studies, to identify new problems in the use of special teachers, and to encourage other schools to experiment with this form of teaching and administration. The conference was made possible by a grant from the Carnegie Corporation of New York.

Canadian Polar Basin Expedition

A 55-man Canadian scientific expedition has begun a broad program of research on Canada's continental shelf on the rim of the Polar Basin. The expedition, which includes oceanographers, topographers, geologists, geophysicists, geographers, and fisheries and wild-life experts, has headquarters at Isachsen on northern Ringnes Island. Ernest F. Roots is coordinator of the project.

Canada knows little about its polar regions. Its arctic shelf, which has not as yet been accurately charted, is about 1500 miles in length and is believed to extend from 100 to 200 miles out into the Arctic Ocean. This year the expedition will cover an area extending 250 to 300 miles out onto the shelf ice from a base line, established by an advance reconnaissance party in 1959, which extends from Meighen Island to Borden Island.

In a systematic study of the shelf, scientists are carrying out various tests at holes blasted through the ice at different points. They are working in a grid pattern across an area 30 miles square. Most members of the expedition will remain in the area until the end of August; all will have left by the end of September.

News Briefs

Growth symposium. Approximately 1000 leading scientists from the United States and abroad will attend the world symposium on the growth of plant and animal life at Purdue University, 16–18 June. The distinguished visitors from other countries will include Harriet Ephrussi-Taylor, France; F. H. C. Crick, Claude W. Wardlaw, John Hammond, and G. E. Blackman, all of England; Jean Brachet, Belgium; E. Zeuthen, Denmark; and Pierre Dansereau, Canada.

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U.S. science education. The National Science Foundation has just published the *Statistical Handbook of Science Education* (NSF-60-13), a compilation of statistical material on the education and training of scientists and engineers in the United States. Copies, at 55 cents each, may be obtained from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C.

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Latin-American physics. The Latin American School of Physics, which met for the first time last year in Mexico City, will meet this year from 27 June to 7 August in Rio de Janeiro, at the Centro Brasileiro de Pesquisas Físicas, under the auspices of Conselho Nacional de Pesquisas do Brasil, Academia Brasileira de Ciências, Campanha de Aperfeiçoamento de Pessoal de Nível Superior, Pan American Union, and UNESCO. The courses, one of which will be conducted by C. N. Yang of the Institute for Advanced Study, will have the general title "Problems of high and low energy nuclear physics." For information, contact J. Leite Lopes, Centro Brasileiro de Pesquisas Física, Av. Wenceslau Braz, 71, Rio de Janeiro, Brazil. In 1961 the Latin American School of Physics will meet in Argentina.

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Biophysical journal. The Biophysical Society and the Rockefeller Institute Press have announced the establishment of the *Biophysical Journal*. The first volume will include eight bimonthly issues beginning with September 1960. Subscriptions will be $8. Biophysical Society members who subscribe through the society will receive a discount. After the first volume, the journal will appear in one volume per year, of six issues, beginning in January, at a subscription price of $6 to nonmembers. Manuscripts and subscriptions are now being accepted. Manuscripts should be addressed to the editor, Professor Frank Brink, Jr., at the Rockefeller Institute, East 66 St. and York Ave., New York 21, N.Y.

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Paper for engineers. A new newspaper entitled *Engineer* has been launched by the Engineers Joint Council (New York), a federation of 21 engineering societies. The quarterly publication was established in recognition of the greatly increasing role of the engineer in national affairs and the fast pace of technical developments that make necessary a medium of communication among all engineers, regardless of speciality.

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For physiology teachers. The Educational Committee of the American Physiological Society is compiling a series of new or unusual experiments that illustrate important physiological concepts. It is hoped that these experiments will help teachers of physiology and human biology in colleges and universities throughout the country. Contributions to the series are urgently needed and will be gratefully received by Dr. C. Haywood, Department of Physiology, Mount Holyoke College, South Hadley, Mass.

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The **Science Teacher Achievement Recognition** (STAR) 1960 program awards were presented at the recent annual convention of the National Science Teachers Association in Kansas City. Fifty-six cash prizes, ranging from $100 to $1000 and totaling $13,500, and more than 70 certificates of honorable mention, were awarded.

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Cell culture genetics. *A Symposium on Cytology and Cell Culture Genetics of Man* (42 pages) has been published by the *American Journal of Human Genetics* with the aid of funds from the National Institutes of Health, Public Health Service, U.S. Department of Health, Education, and Welfare. Reprints may be obtained upon request from Dr. Gordon Allen, Building 10, Room 2N208, National Institutes of Health, Bethesda 14, Md.

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Zoonoses center. A Center for Zoonoses Research will open in September at the University of Illinois. The center, which will be directed by Carl A. Brandly, dean of the College of Veterinary Medicine, will be first of its kind in North America. The only other comparable facility is the Pan-American Zoonoses Center, established in 1956 at Azul, Argentina, and supported by the World Health Organization and the Argentine Government.

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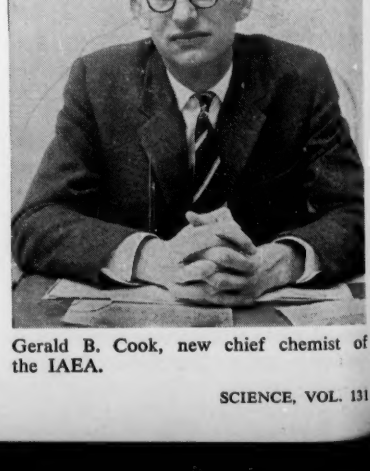
NIH program transfers. Administrative responsibility for the Russian Scientific Translation Program and the Foreign Grants and Awards Program of the National Institutes of Health has been transferred from the Division of Research Grants to the Division of General Medical Sciences under G. Halsey Hunt, chief of the division. The Translation Program is directed by Samuel S. Herman, and the Foreign Grants and Awards Program is headed by R. E. Scantlebury.

Scientists in the News

Gerald B. Cook, well-known English radiochemist, has been appointed chief chemist of the International Atomic Energy Agency laboratory that is being constructed at Seibersdorf, near Vienna. Cook is an atomic energy pioneer, having been closely associated with the chemistry aspects of atomic energy development ever since the initial stages in England, 20 years ago. Since 1947 he has worked at the Atomic Energy Research Establishment at Harwell, first in the Chemistry Division and then in the Isotope Research Division, where he has headed the chemistry group since 1954.



Gerald B. Cook, new chief chemist of the IAEA.

Edward Teller has resigned as director of the University of California's E. O. Lawrence Radiation Laboratory, Livermore, because he wishes to return to teaching and research. He will continue as an associate director of the Lawrence Laboratory and in addition will become professor at large of physics, a state-wide post now held only by Harold Urey of La Jolla.

Teller is succeeded by **Harold Brown**, at present Livermore's deputy director and a scientific adviser to the Air Force and to the Secretary of Defense.

N. Howell Furman and **Eugene Pacsu** of the department of chemistry at Princeton University retire as emeritus professors this month. Furman, Russell Wellman Moore professor since 1945 and former chairman of the department of chemistry, is a past president of the American Chemical Society and consultant to the Atomic Energy Commission. In 1937 he was named first recipient of the Fisher Award in Analytical Chemistry, the highest honor conferred in that field.

Pacsu, who joined the Princeton faculty in 1930, was named in 1947 one of the ten top-ranking men in the field of starch chemistry. Noted for his research in this field, he has been instrumental in making Princeton one of the few training centers in the country for graduate work in carbohydrate chemistry.

Sir **Roger Makins**, chairman of the United Kingdom Atomic Energy Authority, arrived in Washington on 21 May to confer with members of the Atomic Energy Commission, the Joint Congressional Committee on Atomic Energy, and other government officials, and to visit a number of atomic-energy facilities. Sir Roger is accompanied by Sir **William Penney**, member of the U.K. Atomic Energy Authority for Scientific Research.

An invitation to visit this country was extended to Sir Roger in January, shortly after he succeeded Lord Plowden as chairman of the U.K. Atomic Energy Authority. **John A. McCone**, chairman of the U.S. Atomic Energy Commission, has recently made two extensive tours of United Kingdom installations, and 2 weeks ago AEC commissioners **John F. Floberg** and **Robert E. Wilson** visited the United Kingdom laboratories at Harwell and Aldermaston.

Paul W. McDaniel has been appointed director of the Atomic Energy Commission's Division of Research, where he has served as acting director since last summer. As director of the division, McDaniel is responsible for guidance of the commission's programs of basic research in the physical sciences in the national laboratories and for administering research contracts in the physical sciences at many universities and colleges. McDaniel has had 16 years of continuous service with the federal government. His career in the atomic energy field began in 1941, when he joined the Manhattan Project at the University of Chicago Metallurgical Laboratory.

Paul A. Scherer is now on leave of absence from the Carnegie Institution of Washington in order to engage in work for the Research Corporation, New York, related to the colleges and universities of the southeastern United States. He has offices in the AAAS headquarters building.

Edward A. Ackerman, deputy executive officer at Carnegie since 1958, succeeds Scherer. Ackerman was formerly director of the water resources program for Resources for the Future, Inc.

Samuel J. Ajl, program director for metabolic biology at the National Science Foundation, who was formerly associated with the Walter Reed Army Institute of Research, has accepted the position of director of research at the Albert Einstein Medical Center in Philadelphia. Ajl will direct and coordinate the research activities of both the Northern and Southern divisions of the center. He will assume his duties on or about 1 September.

H. Edwin Umbarger, assistant professor of bacteriology and immunology at the Harvard Medical School, has been appointed senior staff investigator at the Long Island Biological Laboratory in Cold Spring Harbor, N.Y.

Frank J. Dixon, professor and chairman of the department of pathology, University of Pittsburgh School of Medicine, will become the first director of the new department of experimental pathology, Scripps Clinic and Research Foundation, La Jolla, Calif., on 1 July 1961.

Also moving to Scripps from the department of pathology at Pittsburgh are **Joseph D. Feldman**, professor of pathology; **Jacinto J. Vazquez**, associate professor of pathology; and **Gordon B. Pierce, William O. Weigle**, and **Charles G. Cochrane**. Dixon, his associates, and their staff will be the first occupants of the new Medical Research Institute of the Scripps Clinic and Research Foundation, which will be completed by July of 1961.

Jack L. Strominger, professor of pharmacology at Washington University (St. Louis) is the recipient of the 1960 John J. Abel Award of the American Society for Pharmacology and Experimental Therapeutics. The $1000 award, which is sponsored by Eli Lilly and Company, was conferred at the annual dinner of the society, which was held in Chicago. Strominger was honored for his work on the mechanism of the action of antibiotics.

Recent Deaths

H. C. Brearley, Nashville, Tenn.; 66; professor, author, and former president of the Southern Sociological Society; wrote *Homicide in the U.S.*; 23 May.

Georges Claude, Paris; 89; inventor of the neon light and pioneer in industrial use of liquid air and of rare gases; 23 May.

Walter G. Elmer, Philadelphia; 88; professor emeritus of orthopedic surgery at the Graduate School of Medicine of the University of Pennsylvania and at the Woman's Medical College of Pennsylvania; 17 May.

Arthur B. Gahan, College Park, Md.; 79; U.S. Department of Agriculture entomologist who retired from the National Museum in 1950; an authority on chalcid flies; 23 May.

Antonie Pannekoek, Netherlands; 87; emeritus professor of astronomy, Univesity of Amsterdam; specialist in study of galactic structure and the history of astronomy.

Pieter Johannes van Rhijn; Netherlands; 74; emeritus professor of astronomy and director, Kapteyn Astronomical Laboratory, University of Groningen; authority on stellar statistics; 9 May.

Erratum: In J. J. Spengler's article, "Population and world economic development" [*Science* **131**, 1497 (20 May 1960)], the heading of the first column in Table 1 (p. 1499) should have read "Gross reproduction rate," not "Gross reproduction rate (%)."

Book Reviews

The Politics of the Developing Areas. Gabriel A. Almond and James S. Coleman, Eds. Princeton University Press, Princeton, N.J., 1960. xii + 591 pp. $10.

From Empire to Nation. The rise to self-assertion of Asian and African peoples. Rupert Emerson. Harvard University Press, Cambridge, Mass., 1960. x + 466 pp. $7.75.

India: the Most Dangerous Decades. Selig S. Harrison. Princeton University Press, Princeton, N.J., 1960. x + 350 pp. $6.50.

The Death of Africa. Peter Ritner. Macmillan, New York, 1960. xii + 312 pp. $4.95.

The four works before us, each with its particular emphasis and approach, are joined in a common effort to explore and interpret the secular processes of political, economic, and cultural change which are transforming non-Western societies at an ever increasing rate. While Emerson and the participants in the Almond-Coleman symposium grapple with the problem in the large, in terms of two and three continents, Harrison and Ritner have chosen a subcontinent and continent as their focus.

Each of these studies, naturally reflecting the special preoccupations and values of the writers, varies in method, organization, and, in some measure, in the nature of the findings. Yet, it is remarkable how much agreement there is on important aspects, however dissimilar the tone or stress. Almond and Coleman, both political scientists (the former noted for his studies of elites and opinion problems and his concern for the comparative method, the latter a highly regarded Africa specialist), have assembled an extremely able team of area specialists to cooperate in the present symposium on the politics and political systems of the "developing areas": L. W. Pye on Southeast Asia, M. Weiner on South Asia, Coleman himself on sub-Saharan Africa, D. Rustow on the Near East, and G. I. Blanksten on Latin America. Emerson, long known as one of the leading students of Southeast Asia and Asian nationalism (with a growing concern for African affairs) has summed up years of inquiry and reflection in this broad-ranging study of the foundations and growth of nationalism, primarily in the Afro-Asian orbit, with an appraisal of its contemporary achievements and problems.

Selig Harrison, combining years of journalistic experience in India with impressive academic qualifications and concerns, has developed a pioneering interest in the role of the language problem in multilingual states. In his present study, unique of its kind in American literature, he probes into the problem of India's multilingualism and its implications as the key divisive agent in the country's political growth. Peter Ritner, a journalist whose interest in Africa was kindled with his assumption of responsibility for a special Africa supplement of a literary journal, here presents the findings of a subsequent field trip and research into the "realities" of contemporary Africa. He addresses himself to the American voter in the passionate conviction that "Something Must Be Done."

Reluctantly overcoming a pragmatist's hesitations, Emerson has embarked on a highly productive "search for uniformities on a grand scale" for which a base is provided by his unifying theme, "the rise of nationalism among non-European peoples as a consequence of the imperial spread of West European civilization . . ." which has "thrust elements of essential identity on peoples everywhere." His reading of the evidence suggests the "inevitability" of the broad results. However dominant its "power and profit" motivation, imperialism became "as radical a transformer of native society" as the "avowed missionary or modernizer." And however inadequate the final "civilizing" achievement, Emerson doubts that, without the impulsions set off by this achievement, the non-European peoples could have been induced to undergo a comparable revolutionary change. Whether or not Westernization and modernization really signified progress, Emerson rests on the "pragmatic ground that the peoples most affected are themselves making such an identification."

The system of foreign dominance has turned out to be self-liquidating, owing to its inherent revolutionary dynamism, which it has transmitted. Yet an "immense imbalance of power," sharp disparities of wealth, and an inadequate understanding of the assumptions underlying Western civilization are part of the transitional societies heritage.

Emerson offers a highly perceptive, generalized sketch of the process of transition, which culminates in the repudiation of colonialism. He concludes that the growth of the new, indigenous nationalism is related much less to neglect and oppression than to the rate of modernization and the rise of Westernized elites which it has stimulated. In this connection, the comparison of British, French, Belgian, and Portuguese colonial attitudes is highly suggestive, as is the writer's astringent appraisal of the role of white settlers. His contention that these groups tend to delay indigenous advance rather than to stimulate it will call forth some spirited argument. Altogether, the colonial system emerges from this appraisal as being neither good nor bad but rather, as if impelled by some Hegelian cunning of reason, invested with a higher liberating purpose which it served despite itself. His acute reexamination of the "nature of the nation" and its main elements will be welcomed by students of this venerable subject, who will find here a helpful critique of the Marxian view in the non-Western context. Nationalism itself is seen everywhere as a "product of the breach with the old order" involving the disruption of traditional groups and loyalties. This is highlighted by the remarkable parallels between the social carriers of that movement in 18th- and 19th-century Europe and in 20th-century Afro-Asian societies.

The wealth of insight and stimulation provided by Emerson can only be suggested here. We should note his

exploration of the connection between colonial and postcolonial democracy, of self-determination, and of some crucial problems of postcolonial societies, more especially his observations on the erosion of democracy in the new states. Sobering views on the problem of foreign aid will be found as a part of the concluding argument. That "economic and social development will surely redound to the benefit of the West" is seen as one of our "cheerful illusions," since its effects on stability and peace are problematic. Aid should nonetheless flow from the determination on the part of the West to associate itself with the processes of revolutionary change which were, after all, set off by Western dominance. This requires "acts of faith" concerning the feasibility and desirability of Western-style growth, on the part of both donors and recipients, in the face of the indeterminacy of the political outcome.

Where Emerson's study is historical-political, with its perceptions and generalizations grounded upon his personal evaluation of a vast body of evidence, Almond and Coleman and their associates have set out to devise a tool of analysis which will permit a "more scientific" treatment of political processes and structures in the non-Western communities. In his elaborate and highly provocative methodological introduction which sets the tone for the undertaking, Almond claims, not unjustly, that this is "the first effort to compare the political systems of the 'developing' areas and to compare them systematically, according to a common set of categories." It is quite true that the conventional categories of comparative government and politics have been largely derived from the experience of Western Europe, that even in the contemporary Western context some of them have become threadbare, and that hitherto only scattered efforts have been made by political scientists to develop categories adequate for the comparison of Western and non-Western systems. Several of the participants in this symposium have been engaged in such efforts, carried on partly under the auspices of the Social Science Research Council. The present undertaking explicitly associates itself with the behavioral approach, "an underlying drift toward a new and coherent way of thinking about and studying politics." This involves certain terminological preferences—such as *political system* for *state*, *functions* for *power*, *roles* for *offices*, *political culture* for *public opinion*, and *political socialization* for *citizenship training*—and generally a leaning toward an esoteric, specialized language which poses problems of communication for all except insiders.

In this context, conceptual shifts, Almond assures us, are "an intimation of a major step forward in the nature of political science as a science." Part and parcel of the "social science explosion," the inquiry leans on the findings of sociologists, anthropologists, social psychologists, and political scientists, among whom Weber, Parsons, Shils, Hyman, Lazarsfeld, Linton, Lasswell, and Truman may be mentioned. Only some of the results can be indicated here. The "political system" is defined sufficiently broadly to fit primitive, traditional, transitional, and revolutionary communities, as well as established states as "that system of interactions to be found in all independent societies which performs the functions of integration and adaptation . . . by means of the employment, or the threat of employment, of more or less legitimate physical compulsion. The political system is the legitimate order-maintaining or transforming system in the society." Stress is placed upon the comprehensiveness of social structures, the interdependence of "subsets," and the existence of boundaries between the political and nonpolitical phases engaged in the process of interactions. It is argued that essentially the same functions (in a variety of patterns) are performed in all political systems, whether "modern" or "traditional." These functions, in turn, are broken down into the "input" functions of political socialization and recruitment, interest articulation, interest aggregation, political communication, and the three "output" functions: rule-making, rule application, and rule adjudication. Throughout it is emphasized that all political systems, Western and non-Western, are transitional and, hence, that dualistic rather than monistic, developmental, as well as equilibrium, models must be used. Almond's essay, remarkable for its suggestiveness and bold originality, is given over to the elaboration and classification of relevant concepts and categories. It breathes a sense of confidence in the vitality and continuing productivity of the chosen approach. The regional studies which compose the bulk of the work, all of them on a high level of professional competence, in turn deal with their subject matter broadly in terms of the functional design projected in the introductory essay. While from the point of view of some readers this may lead to certain artificialities and terminological problems, it gives the work a unity unusual for a collective effort. The regions are treated as units so that discussion of individual countries has been subordinated to the over-all functional treatment for the whole. Coleman concludes the symposium with a notable essay summing up the "modal characteristics" of the several systems and developing the "functional profiles" of systems, based on a recent classification of "new states" by Edward Shils. Clearly, this is a seminal work destined to leave a trail of influence and controversy among students of comparative politics, area specialists, and social scientists in general.

Emerson, Weiner (in his exploration of South Asia), and Coleman (in his concluding analysis) all bring out the gravity of the language problem and its implications for cohesion and continuity, especially in a multilingual society which lacks an actually or potentially dominant language and where other unifying factors are weak. This is the problem that Harrison, with impressive erudition and singlemindedness, explores for contemporary India. Actually, this is a series of studies of crucial aspects of the Indian political system and its background, all of them showing the impact of linguistic and regional loyalties. He reminds us of the old British colonial argument that there never was an India or Indian nation except as a geographic concept, an insult which concealed the continuing question: "Can a unified Indian state in fact survive, and if so, what will be the political price for survival?" The "dangerous decades" of the title refer to the coming period when rising aspirations are astir but before they can be adequately satsfied.

Harrison sets the stage with a preliminary discussion of the principal regional and linguistic traditions of India, which stresses their continuing vitality in a sort of competitive coexistence in the new India. Two further chapters develop, with lucidity and learning, the inevitable decline of English as the language of a national elite since independence, the resistance to the expan-

sion of Hindi as its successor, and the growing emergence of separatist, regional elite groups whose horizon and national cohesion will, in his judgment, be increasingly limited as time goes on. This divisiveness is enhanced by the fact that in the matrix of the old Hindu caste system (which the secularists hoped to wear down) vigorous regional-linguistic caste alignments arise and already have come to wield great power. The argument culminates in a searching examination of the role of the Indian Communist Party which is shown to thrive on the expert manipulation of these divisive factors. It is ironical that not only the ruling Congress Party, but the Communist Party itself, is hampered and riven by the internal working of these forces. While for the present the old remaining national leadership can secure unity, Harrison foresees the possibility of some future authoritarian adventure, as a desperate effort to save the union against the onslaught of separatist forces. There is, of course, the possibility that the tempo of linguistic disintegration will slow down or that external pressures (China) will foster a sense of national unity in leaders and masses. Harrison has contributed a penetrating, truly important study of problems far transcending the fate of the Indian Union itself.

Ritner's *Death of Africa* in a very real sense speaks for itself. He is fascinated by what he regards as the virtually unavoidable advent in Africa of "a historical monstrosity whose whole future is mortgaged to its deformities." He sees in the rapid disintegration of African society a source of coming disasters—social, economic, and political, which partial measures and correctives can do nothing to stem. Only truly massive American support measures can help. This will require a broad reorientation of American economic and foreign policy. To buttress his contention, Ritner takes the general reader on a well-directed guided tour through the principal areas of Africa south of the Sahara. The objectivity of his account is indeed somewhat "adulterated" by his "passionate convictions." But he has something to say, and he tells it with a dramatic force that cannot leave us indifferent. This is a striking and significant brief for a policy change. It deserves an audience.

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Tribes of the Sahara. Lloyd Cabot Briggs. Harvard University Press, Cambridge, Mass., 1960. xx + 295 pp. Illus. $6.

"There has been more pure balderdash written and repeated about the tribes of the Sahara," begins Cabot Brigg's preface, "than about almost any other peoples in the world." No truer words were ever written. The ignorance of most authors, though not their flights of imagination, about the tribes of the Sahara can be excused on the grounds that hitherto there has been no authoritative work in English to which they could turn; and although French literature covers most aspects of human life in the Sahara, that literature is mainly composed of scholarly monographs unsuitable for the general reader. At last we have an over-all picture in our own language, and one which places the French, no less than the English-speaking peoples, under an obligation to its distinguished author, whose polished prose equals his erudition.

The opening chapter is a valuable essay on the geographical factors which impose so narrow and sensitive a margin between life and death on man in the Sahara. In the following chapter the author brings to life the prehistory of the desert in a way that no other writer has approached. But in so doing he is, as indeed throughout the book, at pains to emphasize the limits of our knowledge. He never allows himself the mildest conjecture. This exceptional chapter concludes with a masterly summary of recorded history which for brevity, adequacy, and clarity is quite outstanding.

Cabot Briggs devotes a chapter to each of the main groups of Saharan tribes, in every case selecting for close study a typical member of each group. The Tuareg are represented by the Ahaggar, the Teda by those in the Tibesti, the nomadic Arabs by the Chaamba, and the Moors by the Ouled Tidrarin of the Spanish Sahara. Cabot Briggs protests that his account of the Ahaggar is "full of gaping holes," but it should fully satisfy most of his readers. In his account of the Teda, of whom we know so little, he stirs the imagination by pointing out that the far-distant "Tomb of Tin Hanan" is Teda in character. His accounts of the Chaamba's adoption of shopkeeping as an alternative to raiding and of how the still half-tamed Moors have been known to raid 1500 miles afield and to take 8 months for such raids, make enthralling reading.

Earlier in the book we have chapters on the sedentary tribes and on the hunters and food gatherers, which include revealing sketches of the residual peoples of the Sahara, the tiny obscure tribes whose survival never fails to astonish. Those who have seen, from the security of a ship, the forbidding shore of Cape Bojador can scarcely fail to wonder how Gil Eannes, one of Prince Henry's captains, came to find footprints of men and camels on so desolate a spot. After reading Cabot Briggs, I cannot doubt that the owners of the footprints were the Imraguen, the queer nomadic fishermen who haunt this coast. This discovery is but a small part of my debt to this admirable book. Another is added respect for what the French have done in policing and cherishing these hordes of predatory nomads. There could be no better judge of this than the author.

Finally, a word of praise for the publishers. Excellent type, wide margins, and good maps and illustrations combine to achieve the same high standard as that set by the author. May this book be as widely read as it deserves, and the flood of balderdash be stemmed.

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The Professional Soldier. A social and political portrait. Morris Janowitz. Free Press, Glencoe, Ill., 1960. xiv + 464 pp. $6.75.

Serious inquiry has finally been made into the private life span of the professional military leader as a member of a group. This inquiry has not left a cold, heroic piece of statuary or the memory of an adventuring opportunist to symbolize the type of person to whom we entrust our national security. Instead, it has portrayed a dynamic human figure, continuously in transit from one situation to another, motivated, in the main, by rather altruistic objectives.

While you could not get any one of our officers to agree with all of Janowitz' findings and conclusions, yet the great majority of these officers will admire the depth of inquiry of his study. The *esprit de corps* of each service will generate objections to certain things, ranging from the title of the book itself to what appears, at first, to be

rather singular conclusions concerning each military department. Yet almost all of these conclusions have been expressed at one time or another by individuals as members of the services themselves, even within the particular subordinate organizations under discussion. Thus in many respects, Janowitz has been a chronicler of military viewpoints as well as an analyst in his own right.

Where the author has leveled a criticism—or, perhaps more properly stated, has recorded a criticism leveled against one group, it will also be found that earlier or later he has commended that same group for some other attitude or action. The balance sheet, on the whole, gives all services a good bill of health and gives many pats on the back. In other words, it leaves the service reader proud to belong to the military.

But perhaps the main attribute of the book is that it gives those who have little first-hand knowledge of the military a chance to scrutinize this category of fellow-citizens under clinical conditions. And that is good, because it shows the homogeneity of Americans within and without the service and contributes something to the strength of the military-civilian defense team.

The timeliness of this work is remarkable. It has been published at a moment in United States history when both discerning civilians and military men are asking many questions along the lines of the author's inquiries. At the significant sixth National Military—Industrial Conference (held in Chicago, Ill., 25–27 April 1960, under the auspices of the Institute for American Strategy), a number of the key conference managers and conferees were pleased to learn that *The Professional Soldier* was a prolific source of information for inquiry into such subjects as the so-called "constabulary concept" in outlooks for the military forces in the future; the degrees of involvement in political matters on the part of the military; the comparison of the new "managerial" type of service supervision with the more traditional forms and their present relative acceptance; the reputed ascendancy of the "absolutists" over the "pragmatists" in matters of strategy; the attitude of the services toward creative ability on the part of personnel; the relative promotional opportunities between conformists and individualists; and the comparison of motivation in military men and civilians.

The book is a monument to industrious research and correlation of facts, which resulted in a thoroughly dynamic presentation from start to finish. It inquires into the historical development of myriad problems of management and control in relation to all facets of their environment as a means of assessing the present and the future military capabilities of the United States. Emphasis is given to personnel complexities and to the adaptability of the military as a whole, under the leadership of an elite group, to meet successfully the constantly changing situations of the present era.

The preface concludes that, "Despite its concern with managerial issues, the profession has been able to maintain its heroic posture, in varying degree, and its public service tradition. . . . Civilian control of military affairs remains intact and fundamentally acceptable to the military. . . ." From the military viewpoint it is believed that such conclusions are sound.

It is interesting to note the amount of attention Janowitz has devoted to ideological endeavors in the Armed Forces. In addition to his remarks, it might be said that there appears to be better receptivity to ideological considerations today than there was a few years ago and that this is attributable to the fact that the rank and file are now beginning to come to grips with the actual threat we are facing.

The author "speaks the language" of military strategy and tactics and of political warfare with professional ease. His discourses on the logic of war, on coalition warfare, and on total versus limited warfare and political situations, complexions, and attitudes are extremely knowledgeable and interesting. He has uncovered many nuances in the attitudes and reasoning of his personalities and much fresh background information to substantiate his conclusions.

As a bonus effect, his portrayal of the social conflict attendant on the competition for admission into the elite leadership group (with its observance of service social customs and ceremonies) intrigues the ladies—from those seeking to marry into the service to the dowagers seeking to arrange "suitable matches." Young men will find it valuable for career planning purposes.

I consider the book to be a most important addition to my library.

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Illustrated Flora of the Pacific States. Washington, Oregon, and California. vol. 4, *Begnoniaceae to Compositae.* Leroy Albrams and Roxana Stinchfield Ferris. Stanford University Press, Stanford, Calif., 1960. v + 732 pp. Illus. $17.50.

This series of four volumes represents 60 years of diligent and painstaking study by the authors. This is no mere compilation from the published research of others, hastily thrown together to place a book on the market. It is much more a monograph of the flora of a vast and ecologically diversified region. Doubtless there are small errors of the type inevitably appearing in any major book and omissions of details concerning the occurrence of species in this or that political or minor geographic unit, but basically the work is both original and sound.

Volumes 1 (1923), 2 (1944), and 3 (1951), together with the present volume, form a completed series long awaited by botanists. Roxana Ferris is to be congratulated for completing the series and, thus, fulfilling the dream of Leroy Abrams, whose advancing age and death prevented the appearance of the final volume during his lifetime. The fourth volume, excellently done, is a fitting tribute to an outstanding man.

The final volume includes the major items dependent upon completion of the book as a whole—the keys to the families and the indexes of popular and scientific names appearing in the the four volumes.

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Systema Helminthum. vol. 2, *The Cestodes of Vertebrates.* Satyu Yamaguti. Interscience, New York, 1959. vii + 860 pp. $90.

This second volume of a series designed to present a systematic treatment of all known parasitic worms on the basis of their morphological and, at times, biological characteristics, deals only with the cestodes. The plan and treatment are the same as in volume 1 [*Science* **129**, 956 (1959)].

As in volume 1, the figures are grouped on plates, but frequently the figures pertaining to related genera and species are widely separated, and com-

parison is, therefore, difficult. The figures, all copied from the original works, are small. Some are excellent, but unfortunately many of the original figures were poor, and they have not been improved by reproduction. For details a reading glass is helpful, often necessary.

Diagnoses of the various taxa are clearly stated and appear to be adequate. Typographical errors are relatively few in number. However, on page 432 *Trilobium* is used for *Tribolium*.

This latest classification of the Cestoda inevitably differs in some respects from earlier classifications, the most recent of which is that of Wardle and McLeod, *The Biology of Tapeworms* (1952).

Yamaguti rejects the cestodarian order Biporophyllidea Subramanian (1939) on the ground that *Biporophyllaeus madrassensis* Sub., 1939, appears to be a free proglottis of a tetraphyllidean or trypanorhynchidean cestode.

The ordinal name Lecanicephalidea (syn. Lecanicephala Wardle and McLeod, 1952) is credited by Yamaguti (on page 94) to Baylis (1920). I am at a loss, however, to determine the origin of the name. It does not appear in the only 1920 paper by Baylis dealing with cestodes which Yamaguti lists. Can it be that Yamaguti was misled by a card in the files of the *Index Catalogue of Medical and Veterinary Zoology* at Beltsville, Maryland, on which appears the entry: "Lecanocephalidea [sic] Baylis, 1920, p. 263"? Examination of the paper, here referred to, shows that it deals entirely with nematodes and that the name in question was one which Drasche (1884) had given to a category in which he placed the nematode genus *Lecanocephalus* (= Goezia). The orthography Lecanicephalidea was used by Mola (1928), page 10, line 7, and is evidently a *lapsus* or typographical error for the family name Lecanicephalidae.

In place of the ordinal name Protecocephala Wardle and McLeod (1952), Yamaguti has used the name Proteocephalidea Mola (1928) to contain the family Proteocephalidae. Mola (1928, page 15, line 1), in error (probably typographical), used the spelling credited to him, but in this sentence he was assigning the genus *Ephedrocephalus* to the "famiglia *Proteocephalidea*." However, on page 19 in his system, he used the correct orthography when he placed the family *Protecephalidae* under the order Tetraphyllidea Carus, 1863. In my opinion, credit for the first use of Proteocephalidea as an ordinal name should belong to Yamaguti.

In Yamaguti's system the order Diphyllidea Carus, 1863, is represented by the single family Echinbothriidae Perrier, 1897, with the single genus *Echinobothrium* Beneden, 1849. This genus is considered by Wardle and McLeod (1952) to be a *genus inquirendum* under the order Lecanicephala.

New families erected by Yamaguti in this volume are Cephalochlamydidae and Parabothriocephalidae in the order Pseudophyllidea; Triplotaeniidae in the order Cyclophyllidea; Adelobothriidae and Tetragonocephalidae in the order Lecanicephalidea; and Trilocullariidae and Maccallumiellidae in the order Tetraphyllidea. He has erected three new subfamilies and 26 new genera of which no less than 20 are in the subfamily Hymenolepidinae Ransom, 1909.

It has not been possible to check with care many of the synonymies listed by Yamaguti. However, I am at a loss to understand why the family name Dibothriocephalidae Lühe, 1902, and the generic name Dibothriocephalus Lühe, 1899, which are used by Wardle and McLeod (1952), are not recorded as synonyms of Diphyllobothriidae Lühe, 1910, and Diphyllobothrium Lühe, 1910, respectively.

Despite the criticisms expressed here, this volume on the class Cestoda must be accorded a very high place in the literature relating to this group of parasitic worms. To the serious worker in cestode systematics, it will be indispensable.

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New Books

Biological and Medical Sciences

Berger, E., and J. L. Melnick, Eds. *Progress in Medical Virology*. vol. 2. Hafner, New York, 1959. 240 pp. $10. Contributors are M. P. Chumakov, E. A. Evans, Jr., N. Higashi, R. Koppelman, M. G. Smith, M. Staehelin, M. K. Voroshilova, Th. G. Wardt.

Clark, F., and J. K. Grant, Eds. *The Biosynthesis and Secretion of Adrenocortical Steroids*. Cambridge Univ. Press, New York, 1960. 119 pp. $5. This volume, No. 18 in the *Biochemical Society Symposium* series, contains information on methods of separating, identifying, and measuring the adrenocortical steroids. Recent ideas on the control of the secretion of the adrenal cortex by hypophysis and higher centers also receive attention.

Kelemen, E. *Permeability in Acute Experimental Inflamatory Oedema*. In the light of the action of salicylates. Hungarian Acad. of Sciences, Budapest, 1960. 256 pp.

Lardy, Henry A., Ed. *Biochemical Preparations*. vol. 7. Wiley, New York, 1960. 111 pp. $5.25.

Martin, Phyllis C., and Elizabeth Lee Vincent. *Human Development*. Ronald, New York, 1960. 546 pp. $6.50.

Nicol, J. A. Colin. *The Biology of Marine Animals*. Interscience, New York, 1960. 718 pp. $14.

Pomerantzev, B. I. *Fauna of U.S.S.R. Arachnida*. vol. 4, No. 2, *Ixodid Ticks Ixodidae*. Translated by Alena Elbl. George Anastos, Ed. American Inst. of Biological Sciences, Washington, D.C., 1960. 199 pp. $10.

Stacy, Ralph W. *Biological and Medical Electronics*. McGraw-Hill, New York, 1960. 319 pp. $9.50.

Stecher, Paul G., Ed. *The Merck Index of Chemicals and Drugs*. An encyclopedia for chemists, pharmacists, physicians, and members of the allied professions. Merck, Rahway, N.J., ed. 7, 1960. 1642 pp. $12. Contains approximately 10,000 descriptions of individual substances, more than 3300 structural formulas, and about 30,000 names of chemicals and drugs alphabetically arranged and cross-indexed.

Mathematics, Physical Sciences, and Engineering

Halliday, David, and Robert Resnick. *Physics for Students of Science and Engineering*. Part 2. Wiley, New York, 1960. 524 pp.

Ivall, T. E. *Electronic Computers*. Principles and applications. Iliffe, London; Philosophical Library, New York, 1960. 271 pp. $15.

Lapp, Ralph E. *Roads to Discovery*. Harper, New York, 1960. 191 pp. $3.75.

Lindsay, Robert B. *Mechanical Radiation*. McGraw-Hill, New York, 1960. 423 pp. $10.

Low, William. *Paramagnetic Resonance in Solids*. Suppl. 2, *Solid State Physics*. Academic Press, New York, 1960. 220 pp. $7.50.

Reid, Constance. *From Zero to Infinity*. What makes numbers interesting. Crowell, New York, ed. 2, 1960. 171 pp. $3.95

Seitz, Frederick, and David Turnbull, Eds. *Solid State Physics*. Advances in research and applications. vol. 10. Academic Press, New York, 1960. 531 pp. $14.50. Contributors to this volume: F. J. Adrian, F. De Wit, B. S. Gourary, D. Lazarus, M. R. Schafroth, P. R. Wallace.

Wade, F. Alton, and Richard B. Mattox. *Elements of Crystallography and Mineralogy*. Harper, New York, 1960. 346 pp. $7.50.

Weidner, Richard T., and Robert L. Sells. *Elementary Modern Physics*. Allyn and Bacon, Boston, 1960. 524 pp. $8.50.

Zenz, Frederick A., and Donald F. Othmer. *Fluidization and Fluid-Particle Systems*. Reinhold, New York; Chapman and Hall, London, 1960. 523 pp. $15.

Reports

Search for Artificial Stellar Sources of Infrared Radiation

Abstract. If extraterrestrial intelligent beings exist and have reached a high level of technical development, one by-product of their energy metabolism is likely to be the large-scale conversion of starlight into far-infrared radiation. It is proposed that a search for sources of infrared radiation should accompany the recently initiated search for interstellar radio communications.

Cocconi and Morrison (*1*) have called attention to the importance and feasibility of listening for radio signals transmitted by extraterrestrial intelligent beings. They propose that listening aerials be directed toward nearby stars which might be accompanied by planets carrying such beings. Their proposal is now being implemented (*2*).

The purpose of this report is to point out other possibilities which ought to be considered in planning any serious search for evidence of extraterrestrial intelligent beings. We start from the notion that the time scale for industrial and technical development of these beings is likely to be very short in comparison with the time scale of stellar evolution. It is therefore overwhelmingly probable that any such beings observed by us will have been in existence for millions of years, and will have already reached a technological level surpassing ours by many orders of magnitude. It is then a reasonable working hypothesis that their habitat will have been expanded to the limits set by Malthusian principles.

We have no direct knowledge of the material conditions which these beings would encounter in their search for lebensraum. We therefore consider what would be the likely course of events if these beings had originated in a solar system identical with ours. Taking our own solar system as the model, we shall reach at least a possible picture of what may be expected to happen elsewhere. I do not argue that this is what *will* happen in our system; I only say that this is what *may have* happened in other systems.

The material factors which ultimately limit the expansion of a technically advanced species are the supply of matter and the supply of energy. At present the material resources being exploited by the human species are roughly limited to the biosphere of the earth, a mass of the order of 5×10^{19} grams. Our present energy supply may be generously estimated at 10^{20} ergs per second. The quantities of matter and energy which might conceivably become accessible to us within the solar system are 2×10^{30} grams (the mass of Jupiter) and 4×10^{33} ergs per second (the total energy output of the sun).

The reader may well ask in what sense can anyone speak of the mass of Jupiter or the total radiation from the sun as being accessible to exploitation. The following argument is intended to show that an exploitation of this magnitude is not absurd. First of all, the time required for an expansion of population and industry by a factor of 10^{12} is quite short, say 3000 years if an average growth rate of 1 percent per year is maintained. Second, the energy required to disassemble and rearrange a planet of the size of Jupiter is about 10^{44} ergs, equal to the energy radiated by the sun in 800 years. Third, the mass of Jupiter, if distributed in a spherical shell revolving around the sun at twice the Earth's distance from it, would have a thickness such that the mass is 200 grams per square centimeter of surface area (2 to 3 meters, depending on the density). A shell of this thickness could be made comfortably habitable, and could contain all the machinery required for exploiting the solar radiation falling onto it from the inside.

It is remarkable that the time scale of industrial expansion, the mass of Jupiter, the energy output of the sun, and the thickness of a habitable biosphere all have consistent orders of magnitude. It seems, then, a reasonable expectation that, barring accidents, Malthusian pressures will ultimately drive an intelligent species to adopt some such efficient exploitation of its available resources. One should expect that, within a few thousand years of its entering the stage of industrial development, any intelligent species should be found occupying an artificial biosphere which completely surrounds its parent star.

If the foregoing argument is accepted, then the search for extraterrestrial intelligent beings should not be confined to the neighborhood of visible stars. The most likely habitat for such beings would be a dark object, having a size comparable with the Earth's orbit, and a surface temperature of 200° to 300°K. Such a dark object would be radiating as copiously as the star which is hidden inside it, but the radiation would be in the far infrared, around 10 microns wavelength.

It happens that the earth's atmosphere is transparent to radiation with wavelength in the range from 8 to 12 microns. It is therefore feasible to search for "infrared stars" in this range of wavelengths, using existing telescopes on the earth's surface. Radiation in this range from Mars and Venus has not only been detected but has been spectroscopically analyzed in some detail (*3*).

I propose, then, that a search for point sources of infrared radiation be attempted, either independently or in conjunction with the search for artificial radio emissions. A scan of the entire sky for objects down to the 5th or 6th magnitude would be desirable, but is probably beyond the capability of existing techniques of detection. If an undirected scan is impossible, it would be worthwhile as a preliminary measure to look for anomalously intense radiation in the 10-micron range associated with visible stars. Such radiation might be seen in the neighborhood of a visible star under either of two conditions. A race of intelligent beings might be unable to exploit fully the energy radiated by their star because of an insufficiency of accessible matter, or they might live in an artificial biosphere surrounding one star of a multiple system, in which one or more component stars are unsuitable for exploitation and would still be visible to us. It is impossible to guess the probability that either of these circumstances would arise for a particular race of extraterrestrial intelligent beings. But it is reasonable to begin the search for infrared radiation of artificial origin by looking in the direction of nearby visible stars, and especially in the direction of stars which are known to be binaries with invisible companions.

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Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

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13 May 1960

Automatic Tonometer with Exact Theory: Various Biological Applications

Abstract. Methods for externally measuring pressure within body cavities exist. In tonometer form they are fast and gentle not requiring anesthetics for the cornea of the human eye. Readings are accurate and independent of tissue tension, corneal stiffness, astigmatic curvatures, and surface tension. There are also separate indications of corneal rigidity and relaxation, and tonographic fluid expression. Other applications include monitoring blood pressure, uterine contractions, and infant intracranial pressure.

The classical methods for measuring the pressure within the human eye have involved a measurement of the force required to flatten a given area or a measurement of the corneal indentation produced by a given weight-loaded rod (*1*). Though ophthalmologists learn to make clinical evaluations from such readings, these procedures are somewhat cumbersome and inaccurate. Inexactness is introduced into the readings by corneal effects such as rigidity and the tension in the tissues tending to resist indentation. The magnitude of this effect seems to be rather variable, and thus an uncertainty is introduced into any reading. On the other hand, the surface tension of the tears tends to pull the probing member toward the eye. In the diagnosis of glaucoma it is desirable to observe intraocular pressure alone: this is made possible by the new tonometer that we have devised and demonstrated, since the aforementioned and other extraneous factors are eliminated (*2*). Besides this increased accuracy, the readings are taken more quickly, more gently, and from any position, without the help of expensive auxiliary equipment such as slit lamps.

In Fig. 1 the principle of one form of our device is illustrated. The end of a small, hand-held probe is pressed against the eye. The tip of the probe carries a pressure-sensitive area approximately 1 mm across. If the eye is momentarily flattened to beyond this sensitive area, then, according to first-order theory, the only pressure that will be recorded is the intraocular pressure of the eye. It will be seen from the figure that a high pressure will press down upon the force transducer, which can be a ferrite core that can move toward or away from a coil, thereby varying the coil's inductance. This variation in the position of the core is detected and amplified, causing a variation in current in a coil whose purpose is to exert a restoring force on a small permanent magnet also coupled to the moving system. Thus an increase in pressure will cause an increase in restoring force which will maintain the pressure-sensitive area rather accurately in the plane of the surrounding plate. The measure of force is the current that is recorded as passing into the restoring-force coil. The sensitivity of the detector is 100 mv/μ, and the deflection of the plunger is 0.6 μ for an intraocular pressure of 40 mm-Hg.

The force required to bend the cornea is exerted beyond the sensitive region and is not recorded. For this reason astigmatism and eye size do not enter: the device can be applied with accurate results to the eye of rabbit or man. Tensions in the tissues are tangential forces that are not recorded by the pressure-sensitive area. Surface tension merely pulls the whole probe against the eye a bit harder and does not influence the reading. The end of the probe can be covered by a thin, disposable, sterilizable, rubber membrance without invalidating the reading, but this membrane must be thin, for its presence slightly decreases the sensitivity of the instrument.

The measurement can be recorded or stored electronically in a number of ways. The most convenient and reliable is simply to record the current that measures the force on the sensitive area, as a function of time, on a strip-chart recorder. A representative recording is shown in Fig. 2. As the hand advances the probe against the eye, the indicated reading increases as the flattened region gradually expands to cover the sensitive area. Once the sensitive area is covered, a further increase in total force applied to the probe will cause expansion of the flattened region out over the surrounding plate which then sustains the bending forces which previously acted on the sensitive region. Thus the increasing reading rises to a crest and then drops down into a trough. A further increase in flattened area will raise the intraocular pressure and thus cause a new rise. The crest amplitude is the result of both pressure and bending forces and thus is the reading that is obtained by the classic aplanation tonometers such as that of Goldmann. The reading at the trough is the true pressure reading for the given flattening without the effect of bending forces. Thus the crest-trough difference is a measure of corneal rigidity. It should be mentioned that minor decentering of the sensitive area with respect to the cornea can result in degrading the maximum into a plateau at trough height. In either circumstance measurement from the baseline to the height of the trough or plateau is a measure of intraocular pressure that is absolute and relatively independent of extraneous factors. As the probe is withdrawn the sequence of events reverses itself. The traced out pattern is essentially symmetrical about its center except that the second trough is generally lower than the first. It is assumed that this results from corneal relaxation and a decreased intraocular pressure during the reduction of aplanation following the expression of some fluid. A comparison of dip height advancing and receding measures corneal relaxation, while comparison of trough height advancing and receding measures the rate of expression of fluid during the interval. The height of the central bump in the tracing is a measure of the elevation in pressure due to the tonometric observation. Measurement of the rate of advancement of the probe against the cornea gives the rate of change of volume of the eye which, in conjunction with the rate of change of pressure, yields more information than classical tonography; we call this metrotonometry.

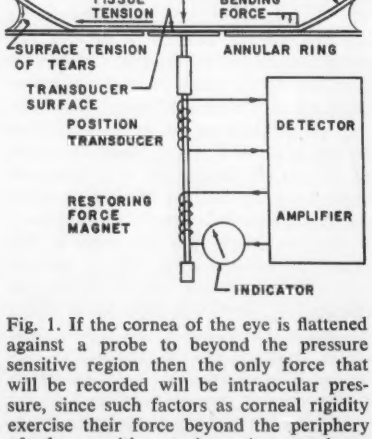

Fig. 1. If the cornea of the eye is flattened against a probe to beyond the pressure sensitive region then the only force that will be recorded will be intraocular pressure, since such factors as corneal rigidity exercise their force beyond the periphery of the sensitive region. Any tendency towards motion in the displacement transducer is detected and amplified to produce a restoring force that holds the pressure sensing piston coplaner with the surrounding region. The restoring-force current is recorded as a measure of intraocular pressure.

In developing this instrument there was some question about the ideal size for the central pressure-sensitive area. Other workers trying to make various extraneous factors effecting the aplanation tonometers cancel out had settled upon a flattened area of 3 mm. Thus our first experiments made use of a

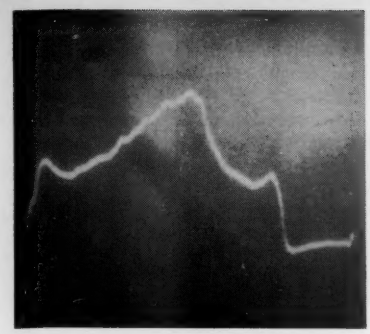

Fig. 2. Representative record traced by a recorder connected in the meter position of Fig. 1 as the probe is advanced against the cornea of a human eye and then withdrawn. The height of the trough measures intraocular pressure and the magnitude of the dip is a measure of corneal rigidity. The first trough indicates 17 mm-Hg, the second, 15 mm, because of the expression of fluid during the interval which was 1 second over-all.

piston 2 mm in diameter and employed a flattening of the eye over a diameter of 3 mm. However, it now seems desirable that both of these diameters be decreased by a factor of approximately two. This is desirable because it is not necessary to cancel extraneous factors which do not enter in the application of this instrument, and because the lessened diameter results in a smaller artificial increase in pressure due to the process of application. To make an accurate interpretation of the cause of the dip in the response curve shown in Fig. 2 it was necessary to perform a series of experiments with specially constructed tonometers having variable piston diameter. From measurements made in this way it was possible to prove that the dip was not caused by buckling of the cornea, in the snap-action fashion of the bottom of an oil can. That is, since the dip was always found to occur at a degree of flattening corresponding to the diameter of the piston, it was proved that the cause of the dip was the expansion of the bent region to a perimeter beyond the margin of the plunger rather than the sudden formation of an inverted vault, which process would be expected to take place at a constant diameter for a given eye or not at all.

Other forms of the tonometer have been tested, and some have proved promising. Thus, forms in which there was no mechanical feedback have been constructed by using a rather stiff force transducer which would record the motion of the plunger while yet keeping the tip of the plunger essentially in the plane of the surrounding area. In any hand-held instrument it is desirable that the mass of all moving parts be kept to a minimum to reduce irregularities introduced by an accelerometer or seismograph effect. Other methods of recording the reading can be employed than the one indicated. Thus electronic circuits can be arranged to store the reading that exists as the current passes through its minimum.

These small probes have many applications in biological experimentation because of their ability to measure intracavity pressure through an intact tissue wall. One of the more obvious examples is the continuous monitoring of blood pressure through the intact wall of a blood vessel. There are sensitivity limitations in every case and these will determine the thickness and stiffness of the wall through which one can measure pressure, and these same factors will influence the most desirable size for the pressure sensitive region. The competing method of performing intracavity measurement is to employ the small swallowable radio transmitters that have been developed in recent years (*3*). But these cannot always be implanted where desired and so the two methods are usually complementary in being applicable to different cases. The primary intention for the present device is to make glaucoma survey more general and routine.

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18 June 1959

Density of the Lunar Atmosphere

John C. Brandt's paper (*1*) contains criticisms of an earlier note of mine (*2*) dealing partly with the same subject, and I have been invited to reply.

It seems to me that Brandt has greatly oversimplified the issue. The tenuity of the permanent atmosphere of the moon is beyond dispute, but I submit that comparatively large amounts of gas may be held by persorption in the porous or pulverous materials, or both, of the lunar surface (*3*), which is in the condition of permafrost at a depth of the order of 1 m (*4*). Since sorption decreases with rising temperature, some of this gas should be liberated by the heat of sunrays, forming a low atmospheric "skin," which is resorbed in the cold of the night. Thus, the lunar atmosphere in the lower selenographic latitudes may yet have in the daytime the ground density attributed to it by Lipsky, even though it is undetectable by Dollfus's method on the night side of the cusp, close to the first quarter, 200 km above the surface of a polar region (*5*).

There are further objections to this method. The gas is assumed to be CO_2 at 0°C, although the ground temperature in these conditions will be −150°C or less, so that most of the CO_2 would have been precipitated. A Wratten 12 filter was used, which suppresses the blue and violet part of the spectrum. Now, the most likely gas to look for in these circumstances is argon, considerable quantities of which should be produced, as was suggested by Shapley, by the decay of the radioactive isotope of potassium. Since argon is monatomic, as against the triatomic CO_2, an atmosphere of this gas will scatter primarily shortwave radiations and appear much "bluer" than one of carbon dioxide. The use of a Wratten 12 filter should make it largely invisible.

Costain, Elsmore, and Whitfield (*6*) have not published, to date, the full particulars of the method by which they estimated the upper limit of the ground density of the lunar atmosphere. Öpik (*7*) supplies some of the missing reasoning, but he is not very explicit either. The electron density will clearly depend on the assumed chemical composition. If the lunar atmosphere is chiefly composed of argon, an inert gas with a high ionization potential, it may not be ionized to any extent in depth, and this might wholly falsify the result. Moreover, on 12 September 1956, Rishbeth and Little observed an occultation of the discrete radio source associated with Kepler's nova (*8*). There was positive response when the source was still 3′ behind the lunar disk, a figure which is in excess of the previous estimates and which may indicate a refraction far above that found in the occultation of the Crab nebula.

For these reasons one should be chary of ascribing too much importance to these negative estimates, the more so as the density of the lunar atmosphere may vary with phase and lunar latitude.

Not having seen Chamberlain's unpublished paper, I am unable to express any opinion on its merits, but I see no reason to dissent from the results quoted by Brandt (*1*). Brandt's mathematical argument has been omitted from the thermofax copy of his report sent to me, but again I am prepared to accept it as correct within the assumptions he makes. It is his assumptions that I find questionable.

The interplanetary medium may con-

sist chiefly of the coronal proton-electron gas having a mean particle weight of ½, as he assumes. Yet it must certainly contain some heavier particles as well, derived, if not from the sun itself, at least from the action of cosmic rays on meteoric matter, from cometary sources, from concentration of interstellar gas in the sun's gravitational field, and, especially in the case of the moon, from the molecular spray of planetary exospheres. The proportion of these particles may be low, but this does not make them unimportant, because the gravitational concentration of interplanetary gas about the moon must be considered as a secular process extended over the whole of the geological time.

Brandt takes into account only what may be termed the instantaneous or differential effect of lunar gravity but leaves its cumulative or integral consequences completely out of consideration.

Let us suppose that the process of gravitational condensation begins to operate at a moment t_0 and that a sample of interplanetary gas, condensed according to Brandt's assumption, comes into contact with the cold body of the moon. It will lose some of its energy and its particles will be substantially slowed down. The next sample of gas, approaching the lunar surface at the moment t_1, will encounter the first sample in its path and be cooled by contact therewith before reaching the moon's surface and experiencing a further chilling. This process will continue indefinitely, the circumlunar gas steadily becoming colder and more condensed, and thus further accelerating the loss of energy by the incoming particles.

After a time the interplanetary particles will no longer reach the surface of the moon, and the zone of intermixture and chilling will move steadily outwards. The temperature of the atmosphere will tend towards that of the subjacent ground, which is shielded from sunrays for a fortnight at a time, indeed, probably dropping below this temperature, since most gases absorb but little of the solar or planetary heat.

This is not the end of the story. Interplanetary gas is a mixture, whose constituents will diffuse outwards; the lighter they are the more readily will they diffuse. Thus, we have here something similar to the process of washing gold dust out of sand. There may be only an ounce of gold dust per ton of sand, but in the end the deposit is almost pure gold. So, too, the atmosphere of the moon gravitationally derived from the interplanetary gas will consist of the comparatively heavy gases which lunar gravity is capable of retaining for sufficiently long periods of time for their loss to be replenished from the surrounding space (that is, if there is no exhalation).

To sum up, the integral result of the gravitational process will be, not an isothermal atmosphere at the same temperature and of the same molecular weight as the interplanetary gas from which it has been condensed inwards, but an atmosphere, isothermal or not, at a low temperature and composed of heavy gases, which has developed outwards from the moon's surface.

How far out could such an atmosphere extend?

This is not an easy question to answer, but the limit of 1000 km assumed by me does not seem unreasonable (*2*). It was my object to make a rough estimate, to the order of magnitude only, of the lower limit for the ground particle density of such an atmosphere. I made what I regard as unfavorable assumptions, such as taking 25 for the mean molecular weight of the atmosphere and 250°K for its temperature, assumed to be the same throughout. As a further precaution, 1000 km was taken for the lunar radius and 150 cm sec^{-2}, for surficial gravitational acceleration. This is a wholly legitimate procedure.

Brandt and I have set ourselves entirely different problems and, therefore, our numerical results cannot be expected to be the same. As regards his closing remarks (*1*), these are so skimpy and obscure that it is difficult to come to grips with them. I have made three points. (i) The density at the escape level of planetary atmospheres, and so the rate of escape, declines with declining gravity (incidentally, my original note contains a regrettable *lapsus calami*. It is stated [*Science* **130**, 1337, col. 2, par. 5 (1959)] that the rate of molecular dissipation will be *inversely* proportional to g, but it is clear from the context that the exact opposite is meant). (ii) If the escape layer is ionized, the free electrons should escape equally to space and to the subjacent layer, producing two oppositely charged atmospheric layers held together by an electrostatic bond. (iii) Because the density at the escape level declines with declining gravity, interplanetary gas will offer increasingly effective collisional opposition to, and diffusive compensation for, the particles escaping from the atmosphere. Theoretically, a point must be reached where dissipation ceases, so that, paradoxically, the atmosphere of a body of sufficiently small mass would become immune to dissipation. I added that this reasoning is not exhaustive.

Brandt says that my first point is "inherent in the conception of the base of an exosphere." Whatever this may mean, somehow or other I have never encountered a clear statement of this situation in the literature on the subject.

My second point is said to be "incorrect," and I am referred to page 306 of *The Sun* by G. P. Kuiper *et al* (*9*), which I have consulted. Page 306 and the following pages relate to the solar corona and not to planetary atmospheres. The gist of the argument is as follows. If m_p be the mass of the proton and m_e that of the electron, $m_p/m_e = 1840$. The two particles carry the same electric charge, of opposite signs, so that electrostatic forces as applied to an electron will be 1840 times more effective than gravity in comparison with the same forces acting on a proton. If protons and electrons are to have the same particle density in an isothermal atmosphere with gravity g, the ratio β of the positive electrostatic force to the gravitational force acting upon a proton must equal 0.5. Then the total restraining force acting on a proton will be

$$m_p g\,(1-\beta) \approx m_e g + m_p\, g\, \beta$$

the latter being the restraining force acting upon an electron. It can be shown that, the restraining force being the same, both types of particle will escape at the same rate. However, this condition cannot be satisfied without a positive electrostatic force as defined above.

Let us now take a planetary atmosphere, to which my argument applies, at night, assuming that this atmosphere's electrical potential is 0 and its ionization is inappreciable. If this gas now emerges into full sunlight and ionization ensues, positive and negative charges, the latter carried by electrons, will be formed in equal numbers, so that they will cancel out and there will be no electrostatic field. Consequently, gravity will operate alone and electrons will escape rapidly to space and to the subjacent layer, as stated, until a sufficient positive charge has been built up to bring about a state of equilibrium. With a weak ionization this state may never be reached, but even if it is, my argument stands: there will be a retaining charge.

Indeed, on the selfsame page 306 van de Hulst himself says, "In an isothermal gas the light electrons have a tendency to segregate from the heavier protons." And if the atmospheric gas is not hydrogen, this tendency will be so much the greater.

Finally, Brandt maintains that my third point is "well known" and refers me to a paper by Öpik (*7*) which contains no mention of it whatever.

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21 March 1960

I have read Firsoff's criticism of my earlier note and I find that no changes are necessary either in my results given there or in my comments relating to an earlier note by Firsoff.

Costain, Elsmore, and Whitfield have published the details of their method of estimating the maximum density of the lunar atmosphere from the radio observations of the lunar occultation of the Crab nebula (*1*). Firsoff is further in error when he states that Rishbeth and Little have observed a response from the radio source associated with Kepler's nova when the source was 3′ inside the limb of the moon. It is explicitly stated in the Rishbeth and Little paper that it was the visible remnant of Kepler's nova that lay 3′ inside the limb of the moon. It is further explained by Rishbeth and Little that the best available position would put the radio source much nearer the limb than the optical remnant.

It seems clear that the kinetic temperature of particles rebounding from the surface of the moon will depend very little on the radiation temperature of the moon, as assumed by Firsoff. Hence, the lunar atmosphere will undoubtly not be at the surface temperature of the moon but will approximate the temperature of the interplanetary gas. The issue is somewhat obscured by Firsoff's manipulation of rather well established astronomical constants such as the mass and radius of the moon, and because he arbitrarily cuts off the lunar atmosphere 1000 km above the lunar surface. Thus it happens that the particle density of $10^7/cm^3$ does not follow from Firsoff's assumptions in a straightforward manner. Firsoff's assumptions given in his original report (which include the claim that the interplanetary density of heavy particles with molecular weight 25 is about $10^8/cm^3$) lead to an atmospheric density at the lunar surface of about $6 \times 10^{17}/cm^3$, a value which is too high by orders of magnitude.

The concept of a critical level or base of an exosphere has been in the literature for years (*2*). A simple definition of the critical level may be given as the region in an atmosphere where a characteristic mean free path is equal to the local scale height. Firsoff's "important factors" (i) and (iii) follow immediately from this definition, and (iii) has also been discussed by Öpik, who considers how the escape rate decreases when the thickness of an atmosphere becomes less than one mean free path. Firsoff's point (ii) is still incorrect, and he is further guilty of quoting van de Hulst out of context. The sentence quoted by Firsoff together with the very next sentence read as follows: "In an isothermal gas the light electrons have a tendency to segregate from the heavier protons. Long ago Pannekoek and Rosseland showed that a minute separation of charge suffices to create an electric field **E** that compensates this tendency by drawing the protons up and the electrons down."

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28 March 1960

Electrographic Evidence of Impaired Brain Function in Chronically Anxious Patients

Abstract. In a study of cerebral function by electroencephalographic techniques the following observations have been made. (i) In intact subjects, repeated stimulation with bright light causes a predictable change (alpha blocking) in the electroencephalogram, whereas repeated auditory stimulation does not. (ii) If, however, an auditory stimulus is presented repeatedly just before the visual stimulus, the sound temporarily but predictably acquires the property of the light to suppress the alpha activity. (iii) This linkage between sound and light occurs much less frequently in human subjects with known amounts of structural brain damage. (iv) A similar electrophysiological defect, implying impairment of brain function, occurs in patients showing severe anxiety during prolonged periods of difficulty in over-all adaptation.

While it is obvious that prolonged periods of poor life adjustment linked with anxiety impair the ability of an individual to function at his most effective level, the demonstration of a defect in brain function by electrophysical means has been difficult to obtain. Studies of the microscopic structure of the nervous system have revealed no significant changes in the brains of persons suffering from the common neuroses and psychoses; studies of the function of the brain as recorded in the resting electroencephalogram have shown only slight, if any, deviation from the normal; studies of the highest integrative functions as evidenced by behavior, attitudes, and thought are limited by the wide variability of motivation and cooperation of such patients.

Early study of electroencephalograms of human subjects showed that repeated stimulation by a bright light predictably provoked disappearance of the alpha activity whereas repeated presentation of an auditory stimulus soon ceased to have any effect on the electroencephalogram. In the 1930's it was noted (*1*) that after a subject's brain waves failed to show a response to an auditory stimulus, if the sound were then paired with a bright light and made to routinely precede the light by a fixed time interval, the sound itself might suppress the alpha activity just as the light had. Such a phenomenon has been known as a temporary cerebral connection or a conditioned cerebral response (it being understood that the phenomenon does not fulfill the criteria for Pavlovian conditioning). These conditioned cerebral responses, for reasons not yet established, are poorly sustained in man.

Since the development of conditioned cerebral responses is a measurable manifestation of brain function which demands minimal cooperation of the subject, and since such responses were found to occur much less frequently in human subjects with impaired function due to brain damage resulting from loss of known amounts of the cerebral hemispheres (*2*), it seemed appropriate to study this phenomenon in patients exhibiting sustained and severe anxiety. Studies were carried out on 23 "control" subjects without evidence of central nervous system dysfunction and on 15 patients who exhibited the consequences of long unresolved adaptive difficulties. They expressed severe anxiety most of the time and conspicuously showed many signs of it. They complained of thinking difficulties, low frustration tolerance, and fatiguability and were to some degree depressed and hostile. Adaptive and compensatory devices were few, poorly developed, and poorly maintained. They were free of the effects of drugs, of infectious, degenerative, neoplastic, or traumatic disease, and of other evidence of gross structural defects of the cerebral hemispheres. Their mean age was 35 as compared with a mean age of 30 for the control group.

The subject was seated in a quiet and semidarkened room and the test procedure was described to him in general terms in order to allay apprehension. Light stimulation was provided by a 150-watt frosted bulb, with a white reflector placed approximately 12 in. from the subject's eyes. Auditory stimu-

lation was produced by a Beltone audiometer, which delivered a sound to one ear at 500 cy/sec and approximately 40 db above the level of audibility. The brain waves were recorded with a Grass model 3 electroencephalograph, frontal, central, and occipital or frontal, temporal, and occipital electrode placements being used. All subjects showed an alpha rhythm present at rest for at least 50 percent of the test period.

The subject was first presented with the light stimulus of 3-sec duration several times to ascertain that light provoked the usual suppression of alpha activity over the occipital regions. This was followed after a brief interval by a sound stimulus of 4-sec duration, repeatedly presented until at least five successive tone presentations failed to suppress alpha activity. The subject was then exposed to paired sound and light stimuli 50 times at irregular intervals, the sound appearing 0.8 to 1.0 sec before the light, with both continuing simultaneously for 3 sec. The interval between the sound and light was automatically timed and remained constant in each individual.

The resting electroencephalogram was evaluated, and the number of conditioned cerebral responses appearing in 50 presentations was ascertained by one of us (C. E. W.) without knowledge of whether the record was obtained from a control subject or from a patient. A conditioned cerebral response was considered to have occurred when the alpha rhythm was obliterated or strikingly depressed following the presentation of the tone and before the appearance of the light.

The resting electroencephalograms of the two groups were indistinguishable. In 23 control subjects the number of conditioned cerebral responses appearing in 50 paired sound-light stimulations ranged from 5 to 16, with a mean of 10.9, a median of 11, and a standard deviation of ± 3.25. In the 15 patients the number of conditioned cerebral responses ranged from 2 to 14, with a mean of 4.8, a median of 4, and a standard deviation of ± 3.28. The difference between these two means is statistically significant, the likelihood of their occurring by chance being less than 1 percent.

These observations demonstrate that the ability to develop conditioned cerebral responses is significantly impaired in a group of patients showing prolonged difficulties in adaptation and severe anxiety when compared with the ability to develop such responses in a group of subjects without obvious impairment of nervous system function. These data are of interest from several standpoints. First, they demonstrate, in subjects suffering from a so-called "functional" nervous system disease without evidence of gross structural abnormalities, failure of the brain, as indicated by its electrical activity, to respond in a normal fashion to external stimuli.

Second, such observations may perhaps help to explain why electroencephalographic studies have been largely disappointing in showing impairment of the highest integrative functions of man. Numerous investigations have revealed only minor differences between the electroencephalograms of normally functioning subjects and those of patients with severe neuroses and psychoses. Perhaps more sensitive methods of measuring responsiveness in the electroencephalogram may demonstrate other evidences of impairment in the "functional" disorders of the brain.

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9 February 1960

Chromosome Number of the Chimpanzee, Pan troglodytes

Abstract. The chromosome numbers of nine chimpanzees (*Pan troglodytes*) have been determined by the bone marrow technique. The diploid number in this species is 48, with a probable XX-XY sex chromosome constitution.

In 1940, Yeager, Painter, and Yerkes (*1*), from an examination of spermatogonial diakineses, reported the diploid chromosome number of a chimpanzee to be 48. No further reports of chromosome numbers in the anthropoid apes have been presented, nor has there been confirmation of the original report since the development of improved cytogenetic techniques. The latter point is of particular interest because of the recent revision of the human chromosome number from $2n = 48$ to $2n = 46$ (*2*).

We have been able to ascertain the chromosome numbers of nine individuals of *Pan troglodytes* (seven males and two females), which were sacrificed because of infection with tuberculosis (*3*). Anesthesia was induced with ether and maintained for 3 to 5 hours with Nembutal. This period afforded sufficient time for the action of the mitotic poisons used: either colchicine, 0.25 mg/kg injected intraperitoneally, or Colcemid (*4*), 6 mg per animal injected intravenously.

Bone marrow was obtained from the proximal third of the humerus (in one case from the radius); it was suspended in 1.12 percent sodium citrate at 37°C for 20 to 30 minutes, centrifuged, and either fixed in cold alcohol–acetic acid and prepared by the Feulgen squash method (*5*) or fixed in 50 percent acetic acid and stained with lactic-acetic orcein and then squashed (*6*). Counts of suitable metaphase plates were made directly from the preparations.

Table 1. Distribution of chromosome numbers.

Specimen		Number of cells scored for various chromosome numbers				
		45	46	47	48	48+
P53	♀	1	1	3	10	1
P60	♀	1	1	4	18	0
P66	♂	1	2	3	8	0
P74	♂	0	0	1	8	1
P79	♂	0	1	4	10	0
P102	♂	0	0	0	5	1
P108	♂	0	0	1	7	1
P118	♂	0	0	1	4	0
N613	♂	0	5	11	58	4
Total		3	10	28	128	8

Table 1 presents the distribution of chromosome numbers obtained in the nine individuals and confirms the observation of Yeager, Painter, and Yerkes (*1*) that the diploid chromosome number in this anthropoid ape is 48.

Figure 1 is a metaphase plate from a female individual (P53) prepared by the Feulgen method.

Comparison of the chromosomes of the sexes suggests an XX-XY sex chromosome constitution, the X being a moderately large metacentric and the Y probably a very small metacentric chromosome. No evolutionary significance can as yet be attributed merely to the difference in chromosome number between man ($2n = 46$) and the

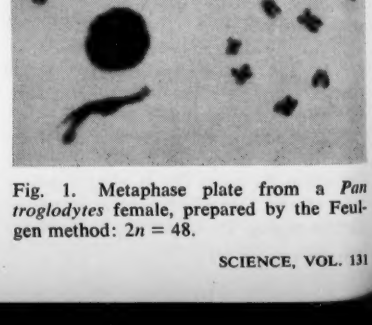

Fig. 1. Metaphase plate from a *Pan troglodytes* female, prepared by the Feulgen method: $2n = 48$.

chimpanzee ($2n = 48$). Knowledge of the significance of the chromosome number of the chimpanzee in terms of primate evolution is dependent, among other things, on a detailed analysis of the karyotype as well as upon the chromosome numbers and karyotype analysis of the other anthropoid apes.

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28 April 1960

Induction of Staminate Flowers on Gynoecious Cucumbers with Gibberellin A_3

Abstract. Staminate flowers induced on gynoecious plants permitted the establishment and increase of inbred lines bearing only pistillate flowers. This method of altering sex expression has practical applications in developing F_1 hybrids, and is useful in studying the physiology and genetics of sex expression.

Completely female (gynoecious) and predominantly female types of sex expression found in certain Japanese and Korean varieties of cucumbers have been recognized as sources of genes that might be utilized to develop female inbred lines for hybrid seed production (*1–3*). Environment and growth regulators alter sex expression in monoecious varieties (*4–6*), and there is wide genetic variability in sex expression (*1–3*, *7*). Stimulation of pistillate flower production with l-naphthalene acetic acid (*4*), and inhibition of pistillate flowers with gibberellin (*6*, *8*), are of special interest in our efforts to utilize genetic variability in sex expression for hybrid seed production.

Beginning in 1954 with a gynoecious segregate found in the Korean variety Shogoin (PI 220860), we developed a number of inbred lines segregating up to 80 percent of gynoecious plants. These inbred lines were used as parents in hybrid seed production by rogueing monoecious segregates from the female parent rows grown in an isolated seed plot provided with a monoecious pollen parent (*3*). This method of seed production is practical and has produced hybrids with a high degree of heterosis. In the U.S.S.R., Mescerov (*2*) independently proposed an almost identical technique for production of F_1 hybrid seed, and likewise observed a striking heterotic effect in his hybrids.

In most of our segregating lines, some predominantly female plants (50 percent or more of the nodes bearing pistillate flowers) were difficult to distinguish from gynoecious plants at the ten-node stage, when seed plots were rogued. The most troublesome were those that developed only one or two staminate clusters after rogueing. This type of predominantly female plant was infrequent, but still a source of some contamination in seed plots. In an effort to eliminate staminate flowers on predominantly female segregates, several chemical growth substances, including gibberellin A_3 (*9*), were applied in 1958 to the foliage of field-grown cucumber plants.

The late developing staminate flowers on predominantly female plants were not completely eliminated. However, gibberellin treatment induced staminate flowers on some plants that would have remained gynoecious. In one line, 17 out of 83 nontreated plants bore staminate flowers compared with 25 out of 33 following foliar applications of 250 parts per million gibberellin. There were 12.4 staminate flowers per 100 nodes on gibberellin-treated plants and 1.3 on the controls.

One completely gynoecious hybrid arising from a cross of gynoecious × predominantly female provided uniform gynoecious plants for further tests in the greenhouse in the fall of 1958. No staminate flowers were produced on control plants, while increasing staminate flower production was observed as the concentration of gibberellin was increased from 250 to 1500 parts per million, and as the number of applications was increased from one to four. Plants receiving four weekly applications of 1500 parts per million produced an average of seven nodes bearing staminate flowers. The induced flowers were normal and produced abundant pollen. Many successful pollinations were accomplished. Homozygous gynoecious inbred lines have been developed through five generations of self-pollination with pollen from staminate flowers induced on gynoecious plants.

Field experiments in 1959 demonstrated a wide range of effective induction treatment and a high tolerance to gibberellin in gynoecious lines. The data from one trial show an increasing response to gibberellin in concentrations up to 5000 parts per million (Table 1). Considerable vegetative distortion followed the 5000 parts per million treatment but many of the plants produced normal lateral branches. No serious vegetative injury resulted from a single application of 2000 parts per million.

In 1959 two 80-ft rows of a gynoecious inbred line, MSU 713-5, were grown in a screen isolation cage provided with a small hive of bees. One row was used to determine the number of foliar applications of 1500 parts per million necessary for adequate pollen and seed production under field conditions. Treatments, replicated three times on four-plant plots, were begun at the second true-leaf stage and ranged from one to four in number. The repeat applications were made at weekly intervals. At least 30 nodes of each plant were examined and classified for sex expression. The nontreated row of 74 plants produced no staminate flowers on more than 2000 nodes classified. A single initial application resulted in nine staminate flowers per 100 nodes and an average of 2.3 per plant. Two, three, and four applications resulted in 45, 66, and 71 staminate flowers per 100 nodes, respectively. Each application affected only two to four nodes per plant, beginning at about node 7 for the first applica-

Table 1. Staminate flower induction by foliar applications of gibberellin A_3 on gynoecious cucumber line MSU 713-21 grown in the field, 1959.

Treatment (ppm)	Total plants	Plants with one or more nodes bearing staminate flowers	Total nodes classified	Total nodes bearing staminate flowers	Staminate flowers per 100 nodes
Control	15	0	417	0	0.0
1000*	14	4	308	7	2.9
1000†	16	8	425	16	4.7
1500*	13	7	283	10	5.3
1500†	16	13	406	27	8.4
2000*	12	6	267	13	5.6
2000†	15	12	359	25	10.9
5000‡	15	10	337	23	9.5

* Applied at first true leaf stage and again 7 days later. † Applied at first true leaf stage and again 14 days later. ‡ Single application at the first true leaf stage.

tion, node 13 for the second, node 20 for the third, and node 27 for the fourth. Intervening nodes bore pistillate flowers and all treated plants reverted abruptly to gynoecious habit. The 118 plants grown in this isolation cage produced 4.2 lbs of seed, demonstrating that ample pollen was provided by two, three, or four weekly foliar spray applications of gibberellin at 1500 parts per million.

In a genetic analysis of sex expression there are obvious advantages in being able to self gynoecious plants in segregating generations and to establish homozygosity for gynoecious habit. In established gynoecious inbreds there is evidence of genetic variability, in that some plants have remained gynoecious even after two applications of 2000 parts per million gibberellin (Table 1). Other gynoecious plants have produced staminate flowers following two applications of 250 parts per million. It is possible to use gibberellin as a selection device to classify plants for ease of staminate flower induction, and subsequently determine how such plants differ genetically.

Sex expression in *Cucumis sativus* can be explained on the basis of response to specific chemical substances synthesized by the plant. These syntheses are controlled by genes. Some genotypes depend upon environment for their expression while others are not sensitive to environment. It is possible that staminate flower production depends upon synthesis in the plant of a gibberellin-like growth substance, and that this synthesis does not occur in gynoecious plants. The establishment of a wide range of sex types under close genetic control should provide material for precise studies of biochemical and physiological mechanisms involved in sex determination (*10*).

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5 February 1960

In vitro Studies of Single Isolated Beating Heart Cells

Abstract. Rat heart cells, separated by trypsin treatment and grown attached to glass in a liquid medium, exhibit periodic contractions similar to a whole beating heart. The rate of beating, which is up to 150 beats per minute, is affected by cardiac drugs and by metabolic substrates and inhibitors.

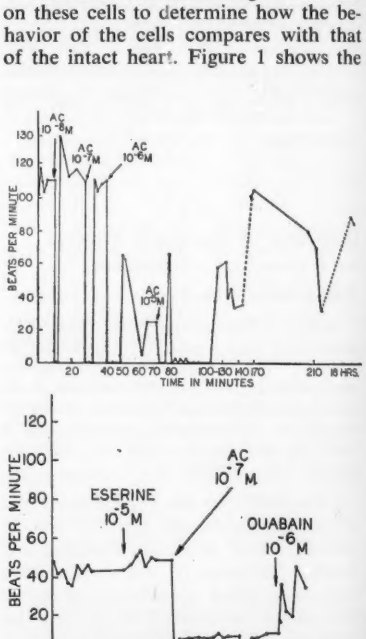
As part of a continuing study of the process of so called "dedifferentiation" in mammalian cell cultures, the cells of the rat heart were separated and grown in vitro. Upon examination of the cultures, it was seen that several of the separated cells exhibited a distinct and rhythmic contraction similar to a heart beat. Some of these cells were observed to beat for as long as 40 days.

The cultures were prepared by separating the cells of coarsely minced young rat heart by incubation with 0.10 percent trypsin for 30 to 60 minutes at 33° to 36°C (*1*). The resultant cell suspensions were centrifuged and suspended in a modified Puck's medium (*2*). The suspensions were made dilute enough so that contact between cells in the cultures was minimized. At first the cells were spherical, but within a few hours many of the cells settled to the bottom of the petri dish and became attached to the glass.

After 3 days at 37°C in an atmosphere of 5 percent CO_2 and 95 percent air, the cells appear to be either typical fibroblasts or myofibrils. Both of these types have been observed to beat. The cells are attached at first by a simple flattening at the bottom cell surface. Eventually the cells may flatten entirely to form either a symmetrical or an irregular shape. Long ameboid or thin filamentous processes increase the irregularity in the shape of the cell. The upper surfaces of the beating cells bulge upwards spherically or are stretched flat between what appear to be the strongly attached processes. It should be emphasized that the morphology of these cells varies and that no correlation can be made between their ability to contract and their anatomy. The lack of a definite histological identity, however, does not alter the fact that they are clearly functional heart cells. Most of the cells contain only one nucleus, although some multinuclei have been observed. In most cases only a small proportion of the cells have been observed to beat and the beating ranges all the way from intermittent, irregular twitches to steady, deep, rhythmic contractions at rates up to 150 per minute. Most of the cells, however, are within the range of 30 to 80 beats per minute. When two or more single beating cells have been observed in the same microscopic field they appear to beat independently.

During the first few days the increase in area covered on the glass seems to be due to the spreading out of the cells. Within a week an increase in the number of cells occurs. After three transfers, in which dilute trypsin was used to detach the cells from the glass, the suspensions were counted in a hemocytometer. An over-all fourfold increase in the number of cells has been observed. After transfer of these cells to another dish with enough dilution so that separate cells were again seen, no beating cells were found, and at no time in any of the transferred cultures were beating cells found.

The effect of certain drugs was tested on these cells to determine how the behavior of the cells compares with that of the intact heart. Figure 1 shows the



Figs. 1–3. Effect of cardiac drugs and metabolic substrates and inhibitors on rate of beating of rat heart cells. The substances were added at times shown by arrows, with final concentrations indicated. Fig. 1 (top), effect of acetylcholine (AC). Fig. 2 (middle), effect of eserine and AC. Fig. 3 (bottom), effect of adenosine triphosphate (ATP).

results of treatment with acetylcholine. A concentration of $10^{-6}M$ stopped the beating completely for 4 minutes. Upon recovery the beating increased about 30 percent over the original rate, then dropped to the original rate. Further addition of acetylcholine eventually led to a marked decrease in the rate, followed by recovery.

In Fig. 2 it is seen that eserine had no effect on a cell which beat at a rate of 50 times per minute. Addition of acetylcholine lowered the beat to a rate of 8 per minute. This rate was maintained until ouabain was added, at which time the rate was raised to about 30 per minute.

The recovery of the noneserinized cells after inhibition with low levels of acetylcholine may be a result of the release of inhibition by the action of cholinesterase. Thus the inhibition of the recovery by eserine could be due to the inhibition of cholinesterase which might not now release the inhibition by acetylcholine.

Several attempts are being made to determine the energy source in the heart cell, necessary for beating. Dinitrophenol (DNP), known to uncouple oxidative phosphorylation (*3*), was added to the medium at various concentrations. At a level of $5 \times 10^{-5}M$, after a short transient stimulation, the rate fell from 90 to 10 beats per minute. Since DNP is thought to act specifically by uncoupling phosphorylation of adenosine diphosphate (ADP) to adenosine triphosphate (ATP), the latter compound was added to the inhibited system. ATP at a concentration as low as $5 \times 10^{-5}M$ restores the rate to 30 to 40 beats per minute. At $5 \times 10^{-6}M$, ATP shows the greatest effect, increasing the beats to 100 to 150 per minute.

The effect of increasing concentrations of ATP is shown in Fig. 3. Not till $5 \times 10^{-6}M$ was reached was any effect noticed. The rate jumped from 60 up to 240 beats per minute, and this was followed by an inhibitory phase and then recovery to the initial rate. Similar experiments with ADP and adenosine monophosphate (AMP) at the same concentrations showed no effect. However, when the concentration reached $10^{-4}M$, ATP, ADP, and AMP all inhibited completely.

The effect of other metabolic inhibitors was studied. Monofluoracetate at $10^{-6}M$ inhibited completely, which indicated the importance of the tricarboxylic acid cycle (*4*) for the periodic contractions. Iodoacetamide at $10^{-3}M$ also inhibited the beating. This high concentration may affect the cell by binding sulfhydryl enzymes other than triosephosphate dehydrogenase. 2-Desoxy-D-glucose at $10^{-3}M$, which has been reported to inhibit glycolysis (*5*), had no effect. However, in the presence of the large amount of glucose present in the media, this concentration may not have been high enough to compete successfully.

These single beating cells isolated from rat heart may provide a unique system for the study of the requirements of the periodic contractility typical of mammalian hearts. Particularly, they may provide a means of determining the contribution of various metabolic pathways for the process, and for determining its nutritional requirements.

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A Study of Thermoregulatory and Emotional Sweating in Man by Skin Ion Transfer

Abstract. Local introduction of atropine and dibenzyline into human skin was carried out by iontophoresis. Both thermoregulatory and emotional sweating were blocked by atropine but were not blocked by dibenzyline. It would seem that emotional sweating produced as a result of a physical stress situation is partly or predominately under cholinergic control.

It has been established that thermoregulatory sweating is mediated through cholinergic response of the sweat glands (*1*). Sweating can be produced by the systemic administration of cholinergic drugs and can be inhibited by the systemic administration of the belladonna alkaloids, with a significant rise in body temperature in man. Little is known, however, of the sweat glandular response to emotional stress. It has been a clinical observation, since the introduction of the adrenergic blocking agents, that the systemic administration of these agents will suppress the hyperhydrosis of the hands and feet of emotionally labile people.

The term "adrenergic blocking agents" designates those compounds which selectively inhibit the responses of effector cells to adrenergic sympathetic nerve impulses, and to epinephrine and related amines. The locus of action of blocking agents of this type is on effector cells, and is selectively distinguished from that of substances which prevent sympathoadrenal discharge by blocking nerve impulse transmission in autonomic ganglia, along peripheral nerves, or within the cerebrospinal axis.

Whether it is the adrenergic rather than the cholinergic response of the sweat glands which produces emotional sweating has not yet been established.

Introduction of atropine and dibenzyline into human skin was carried out by iontophoresis (*2*) in order to affect the sweat glands situated in the skin over a localized area. An aqueous solution of 0.25-percent atropine sulfate and 0.1-percent dibenzyline hydrochloride in 20-percent propylene glycol was freshly prepared each test day. Water was added to the solvent to facilitate ionization of the material. The positive electrode was used in each case, 10 ma for 20 minutes for atropine and 10 ma for 40 minutes for dibenzyline, over an area of 30 to 40 cm^2 of body surface. The much longer time used for the introduction of dibenzyline was found to be necessary in order to assure proper introduction. Testing was done 1 hour after introduction of atropine and 3 hours after introduction of dibenzyline.

Proof of introduction into the skin of dibenzyline was established by the intradermal injection of 0.1 ml of 1:1,000,000 epinephrine hydrochloride into the areas of iontophoresis and demonstration that the local skin blanching (*3*) at the site of the injection was not present over the dibenzyline treated areas, as opposed to the nontreated opposite part serving as a control. It was not deemed necessary to do similar testing with atropine, since a systemic reaction to atropine was observed in two patients, which in itself served as proof of introduction. No such systemic reaction was observed with dibenzyline. Electrophoresis of propylene glycol without the addition of dibenzyline was also carried out to be sure that the solvent had no blocking effects. None was observed. Sweat patterns were identified by the application of the established iodine-starch method. Five patients were tested, two males and three females.

In the thermoregulatory sweating trials both atropine and dibenzyline were alternately introduced into the volar surfaces of the forearms. The iodine-starch technique was applied to the areas treated. The patient was then covered with blankets and allowed to remain in an extremely warm room for

30 minutes until sweating was pronounced.

In the emotional sweating trials both atropine and dibenzyline were alternately introduced into the palms. The iodine-starch technique was applied to the areas treated. The patient was put in an air-conditioned, cool room for 30 minutes as a control period to be sure that no thermoregulatory sweating took place. A painful stimulus was then applied, either by performing a clumsy venipuncture or manipulating a body part until obvious pain and sweating was produced.

The following observations resulted from the above trials: Well delineated sweat patterns were observed on the areas tested after overheating and after stress. In all instances, both thermoregulatory and emotional sweating were blocked by the local introduction of atropine into the skin. In no instance was either thermoregulatory or emotional sweating inhibited by the local introduction of dibenzyline into the skin.

The sweat glands are anatomically under control of the sympathetic nervous system, but their function is modified by drugs which act on parasympathetically innervated effector cells. This was clarified by Dale and Feldberg (*4*) in 1934 when it was discovered that nerve impulses which cause sweating release acetylcholine at the neuroglandular junction. Although the nerve fibers involved traverse sympathetic pathways, they are functionally analagous to parasympathetic nerves. In man, most sweat glands are probably innervated by fibers which function via acetylcholine as the mediator, but there is indirect evidence that the sweat glands in certain areas may be supplied by fibers which release the sympathetic mediator at the periphery (*5*). Haimovici (*6*), in 1950, pointed out that sweating in man can be elicited by adrenergic agents and can be inhibited by an adrenergic blocking agent. He concluded that in addition to the known cholinergic fiber supplying the sweat glands, there is also an adrenergic component in the nervous mechanism of sweating in man.

Accordingly, there can be little doubt that there is both cholinergic and adrenergic response of the sweat glands under certain circumstances, but under the conditions existing during my experiments it seems that both thermoregulatory and emotional sweating remains under cholinergic control. There may be a quantitative difference in the type and amount of sweating, depending on the emotional state of the individual. Perhaps during a prolonged emotional stress period the adrenergic mechanism becomes more prominent, and this may account for the clinical observation that adrenergic blocking agents will control sweating to some degree (*6*).

Dibenzyline is thought to act as a blockade to one of the steps in the process of excitation by adrenergic agents. This blockade is interposed between the penetration of the cells of the effector organ by norepinephrine and secretion of the effector organ. If adrenergic sweating does in fact exist, perhaps the site of action or the mechanism of excitation at the effector organ is not as previously thought. Perhaps the site of action of systemic dibenzyline is different than that produced by local iontophoresis. More definitive work is necessary to further clarify this mechanism.

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4 February 1960

Movement of Radiosodium in a Chemically Stratified Lake

Abstract. The rapid horizontal dispersal of sodium-24 at an average rate of about 18 m/day has been observed near the bottom of a small thermally and chemically stratified lake. However, no appreciable vertical movement of the radioactivity was observed during a period of 6 days.

The discovery of a meromictic lake (Stewart's Dark Lake) in northwestern Wisconsin (T.33N., R.9W.) has provided us with the opportunity to study the limnological characteristics of meromixis. Moreover, we envisaged the use of this lake for pilot studies in aquatic science pertaining especially to problems of radioactive waste disposal.

Meromixis occurs in a lake in which the dissolved substances create a gradient of density differences, in depth, preventing complete mixing or circulation.

This type of lake is normally stratified in three arbitrary zones. The upper, freely circulating layer of water is termed the mixolimnion (*1*). The bottom, relatively very dense, noncirculating stratum is the monimolimnion (*2*) and the transition zone between, the chemocline.

Most temperate-zone lakes are stratified throughout the summer and winter because of density differences owing to a temperature gradient. However, during the spring and autumn when temperatures become homoiothermal from top to bottom, relatively low wind velocities can cause these lakes to "turn over" or circulate completely. Meromictic lakes may have a thermal stratification superimposed upon the chemical stratification. Nevertheless, it is the solute concentration that maintains the stability which persists from year to year, thereby inhibiting the intermingling of the monimolimnetic waters with the above water.

Stewart's Dark Lake is a bog lake with an area of approximately 2 acres. The maximum depth is 8.8 m, the average depth 4.3 m. The lake is maintained by seepage, for there is no inlet or outlet. Colloidal (humic) materials arise from decaying vegetation and "stain" the water a dark brown. The lake represents a highly restrictive habitat for organisms because the monimolimnion never contains measurable amounts of dissolved oxygen. This condition becomes extreme during the winter when the entire lake is characterized by the absence of dissolved oxygen for 2 months or more. In addition, the lower strata contain relatively high concentrations of sulfides.

From the limnological data obtained on the meromictic nature of this lake during the past 2 years, it was observed that under optimum conditions for "overturn," complete circulation extended only to the 6-m level. Thus the monimolimnion, by definition, exists continuously in at least the bottom 2.5 m of the lake. This represents 12.7 percent of the total volume of the lake. Carbonate levels as high as 96 mg/liter persist in the deep water as compared with a concentration of 5 mg/liter at the surface.

In order to obtain information concerning the extent to which this supposedly "stagnant" zone is isolated from the remainder of the lake, we used a radioactive tracer. On 1 July 1959 approximately 47 mc of sodium-24 in the form of NaCl in HCl solution were released within the lake at the 8-m level by smashing an 800-ml museum jar containing the radioactive solution.

Sampling was done along six transect lines radiating from above the release point. These were lines of polyethylene "floating" rope which spanned the lake.

A 2-inch sodium iodide crystal scintillation detector, enclosed in a watertight

Lucite container, was used. This apparatus was lowered by means of extended cables to various depths and locations within the basin. This method of detection plus the Na^{24} was used in an attempt to alleviate some of the sampling difficulties alluded to by other workers (*3*).

Figure 1 is an outline map of Stewart's Dark Lake showing the approximate extent of horizontal movement of the Na^{24}. The radioactivity was dispersed horizontally from the release point in a somewhat symmetrical pattern and was observed to have reached an average distance of 18 ± 3 m in all directions at the end of the first 24 hours. In one direction, a maximum horizontal movement of 22 m was observed during this period. This is a rate of horizontal movement, in this relatively static zone, which is several times greater than that found by other workers in similarly stable lake strata (*3*, *4*).

The radioactive material flowed along the bottom contour and continued to move toward the shore, reaching an average distance of 24 ± 3 m from the release point in 48 hours. Owing to the rapid decay of the Na^{24} and its dilution within the lake, however, accurate determination of the movement after the first day was difficult and became impossible by the third day. In addition, the background emanating from the bottom muds was highest near the shore, further hindering the accurate determination of the leading edge of the radioactivity.

Careful observations were made to determine whether the radioactivity was transported vertically within the lake. No appreciable vertical movement was observed above the release point, except shortly after the discharge, when high concentrations were found at the 4- and 6-m levels. This initial upward movement may have resulted from a density difference between the radioactive solution and the lake water at the release position. Also, a few small bubbles were seen to come to the surface as the bottle was broken, thus possibly transporting some radiosodium to higher levels. Further upward movement of the radioactivity, after this initial movement, was not observed above the release point. In addition, no vertical movement of the radioactive material was found at any other location in the lake.

The radioactivity was detectable in the lake for only 6 days, a fact which minimized any possible problems of radiation hazard to wildlife and other aquatic organisms.

There are at least two possible explanations for the rapid horizontal movement of the radiosodium; (i) physical transport as influenced by eddy diffusion, currents, internal seiches, and (ii) biological transport of the radionuclide to other parts of the lake (*5*).

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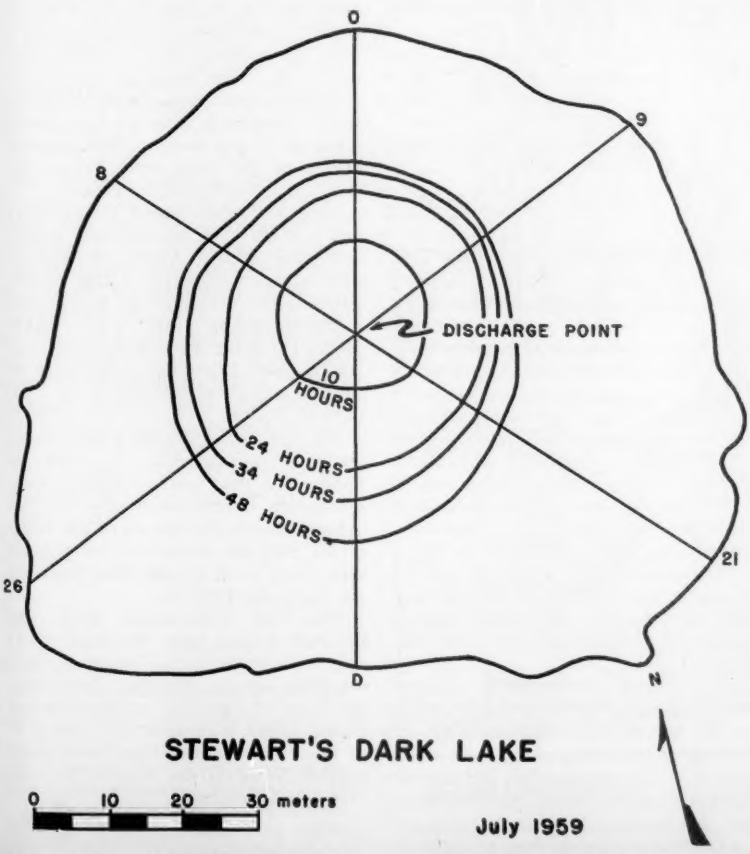

Fig. 1. Sodium-24 was released near the center of the lake at a depth of 8 m. The outlines mark the approximate horizontal movement of the radioactive material.

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22 January 1960

Continuity of Mid-Oceanic Ridge and Rift Valley in the Southwestern Indian Ocean Confirmed

Abstract. The existence of a continuous, rifted, mid-oceanic ridge in the southwestern Indian Ocean, previously predicted by us, has been confirmed by soundings taken by the research vessel *Vema* during the expedition now in progress.

An important objective of the present world cruise of the *Vema* (*Vema* cruise No. 16, 1 October 1959 to 1 August 1960) is the examination of the Mid-Oceanic Ridge. The outstanding questions are (i) whether the ridge is continuous, as has been inferred from topographic and seismicity data (*1*, *2*) and (ii) whether the median rift, which has been shown to coincide with the epicenter belt along the Mid-Atlantic Ridge (*3*), follows the epicenter belt along the entire ridge system. The scarcity of soundings in the southern Indian Ocean has made this a critical area for checking the prediction that the mid-oceanic epicenter belt is a guide to the continuation of the median rift valley through unsounded areas.

Table 1. Principal topographical data obtained on six crossings of the mid-oceanic epicenter belt in the southern Indian Ocean and on three crossings in the Atlantic Ocean.

Crossing No.	Latitude and longitude	Depth (fathoms)			Width (mi)	
		Valley		Highest point on profile	Bottom	Top
		Top	Bottom			
1*	16°35′N, 46°25′W	1180	2600	1140	5	18
2	7°50′N, 38°55′W	1850	3120	1580	4	14
3	18°40′S, 12°50′W	1360	1940	970	5	18
4	47°20′S, 31°47′E	1540	2635	680	1	20
5	39°30′S, 45°00′E	1140	1990	820	5	20
6	37°56′S, 49°08′E	880	2040	750	5	17
7	33°46′S, 56°07′E	1050	2000	655	1	15
8	28°49′S, 61°52′E	1860	2760	1115	1	12
9	30°27′S, 76°26′E	1550	2100	1130	½	4

* Same location as 1938 *Meteor* crossing (*8*).

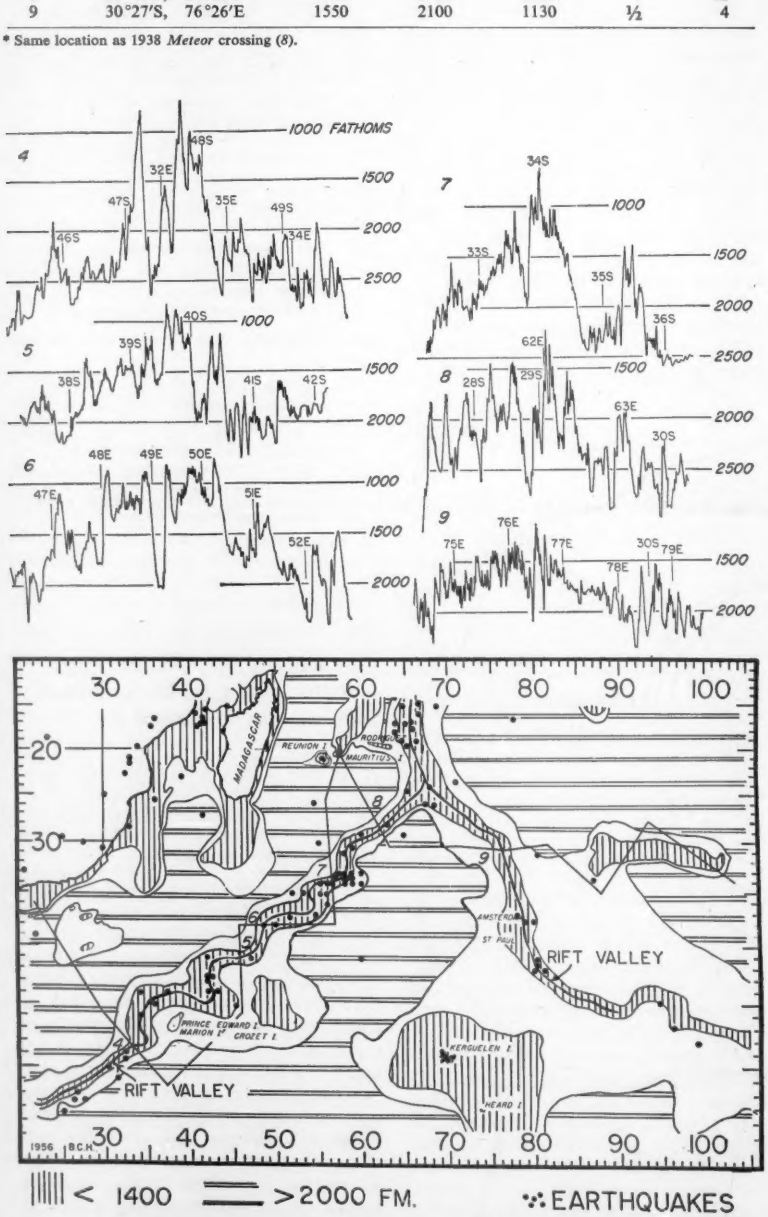

Fig. 1. (Top) Topographic profiles and (bottom) a bathymetric chart of the southern Indian Ocean. Data were obtained by the *Vema* expedition in crossings Nos. 4 through 9 of the mid-oceanic epicenter belt. Vertical exaggeration of profiles 100:1, depths in fathoms at sounding velocity of 800 fathom/sec.

During *Vema* cruise 16, six crossings of the mid-oceanic epicenter belt have been made in the southern Indian Ocean (Fig. 1) and three in the Atlantic Ocean. The principal facts about the topography that was found on these crossings, taken from preliminary readings and radio reports, are listed in Table 1.

The question of continuity of the ridge in the southwest Indian Ocean is of particular interest. The belt of earthquake epicenters, which follows the crest of the Mid-Atlantic Ridge throughout its length, continues without interruption past Prince Edward Island, through Rodriguez Island, and into the Gulf of Aden (*4*). Almost all published bathymetric charts (*5*) fail to show a corresponding ridge. Although a segment trending northeast from Prince Edward Island has been recognized, a major gap is generally shown between Prince Edward Island and the Mid-Atlantic Ridge. However, crossing No. 4 found the ridge fully developed, as had been predicted by Ewing and Heezen (*1*). Crossings Nos. 5 and 6 likewise found the ridge fully developed, as would be generally expected.

Southwest of Rodriguez Island the epicenter belt is continuous, but soundings are rare and no ridge has generally been shown over a span of about 1000 miles (*5*). The track of *Vema* was chosen to explore this region. Crossings Nos. 7 and 8 in this region may be taken as strong evidence for the continuity of the ridge from Prince Edward Island to Rodriguez Island.

Near Rodriguez Island, the epicenter belt branches; the southeastern branch continues through Amsterdam and St. Paul islands toward the Pacific Ocean. Crossing No. 9 verified the existence of a corresponding ridge near latitude 30°S. The three Atlantic crossings only confirmed our previous knowledge of the continuity and position of the Mid-Oceanic Ridge.

All nine crossings also give evidence about the median rift. As shown in Table 1, a deep narrow valley exists, coinciding exactly with the belt of epicenters. The dimensions of the valley on all crossings are closely comparable with those given for the Rift Valley in the North Atlantic (*3*).

Thus, the Mid-Oceanic Ridge and the Rift Valley have the same characteristics in the Indian Ocean as those described for the Atlantic. Apparently the form of the ridge changes before Easter Island is reached (*6*), and it is of interest to find how far the characteristics found in the Atlantic and Indian oceans continue (*7*).

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23 February 1960

Succinylcholine and Muscle Excitability

Abstract. Succinylcholine lowers the resting membrane potential taken with microelectrodes similarly in nerve-scarce and innervated portions of frog sartorius muscle. Twitches to electrical excitation of the nerve-scarce pelvic end of the muscle are also rapidly reduced. The results indicate that succinylcholine probably acts generally on the muscle membrance to diminish excitability.

It is generally believed that the class of neuromuscular blocking agents known as "depolarizing blockers" act by lowering the membrane potential in the end-plate region, thereby making neuromuscular transmission ineffective (*1*). Another theory of action is that these agents decrease the sensitivity of the end-plate region to transmitter agent (*2*). Recently we found, with a microelectrode technique, that some of these agents (acetylcholine and choline) diminish the resting membrane potential, generally to the same degree, everywhere in the isolated frog sartorius muscle membrane. The potential fell to the same extent in and out of the end-plate region, and the effect was prevented by pretreating with curare (*3*). Decamethonium and other agents in this group also caused a generalized fall (*4*). Inexcitability to direct electrical and mechanical stimulation of the muscle was roughly correlated with the mean degree of membrane potential diminution and the amount of blocking agent added to the bath. The hypothesis was advanced that a generalized action of these agents and of curare occurs everywhere on the membrane of the muscle.

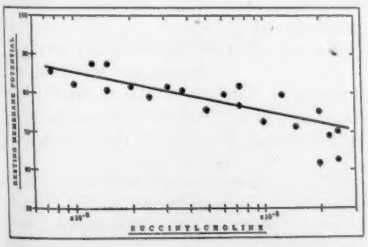

Fig. 1. Resting membrane potential and succinylcholine. Each point represents the mean resting membrane potentials of 50 to 55 fibers, taken with microelectrodes from frog sartorius muscles after adding succinylcholine in concentrations (gm/cm³) shown on the abscissa. Resting membrane potential is shown on the ordinate.

Attention was turned to other agents classified as depolarizing blocking drugs; the effect of succinylcholine is of particular interest. Single fibres were sampled at different sites along the length of frog sartorius muscles by the Ling-Gerard microelectrode technique as previously described (*3*). Each point in Fig. 1 represents the mean of approximately 50 to 55 fibers for a given muscle determined after adding different concentrations of succinylcholine. Lower mean membrane potentials were found with increased amounts of this agent. Pretreatment with curare prevented the fall in membrane potential caused by succinylcholine.

The effect of succinylcholine on the isotonic-twitch response is shown in Fig. 2. For these experiments, frog sartorius muscles were clamped and directly stimulated maximally at the pelvic nerve-scarce region with 5-msec pulses at 15 second intervals. Adequate controls showed that current did not spread to nerve endings outside the pelvic nerve-scarce region.

When succinylcholine was added to make a final concentration of 5×10^{-6} gm/cm³ (Fig. 2*A*) or 20×10^{-6} (Fig. 2*B*), the mechanical twitch response to direct electrical stimulation showed a characteristic diminution. The decrease appeared after a latency which was shorter with higher concentrations of succinylcholine (compare *A* and *B*, Fig. 2). A contracture seen as a rise in base line just after adding succinylcholine (Fig. 2*B*) was related to the amount of drug added. An occasional twitch larger than normal was also common at this time (Fig. 2*A*). The amplitude dropped, reaching a "plateau" of smaller twitch response heights which was not directly related to the concentration of the agent added. After pretreatment with curare (6×10^{-6}), succinylcholine action was blocked (Fig. 2*C*).

Relatively small drops in membrane resting potentials were found with amounts of succinylcholine which were

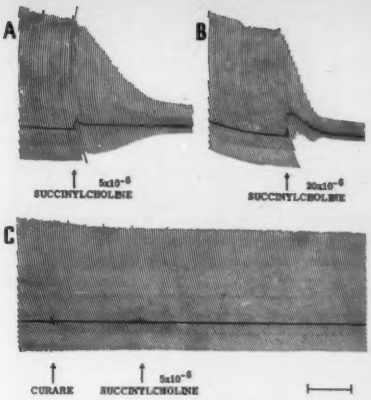

Fig. 2. Twitch responses and succinylcholine. Twitch heights to maximal direct stimulation at the relatively nerve-scarce pelvic end of isolated frog sartorius muscle are shown. Stimulation every 15 sec with 5-msec pulses. *A*, after 5×10^{-6} (gm/cm³) succinylcholine; *B*, 20×10^{-6}; *C*, curare 6×10^{-6} added and then 5×10^{-6} succinylcholine ineffective. Bar represents 5 min.

effective in diminishing direct excitability. This suggested that succinylcholine does not produce its block of excitability by simply lowering resting membrane potential. Jenerick and Gerard had shown that the sartorius muscle membrane could support an action potential until the resting membrane potential was lowered by KCl to a critical level of 52 to 57 mv (*5*). The inference drawn of an excitation block applies as well to similar data obtained with acetylcholine, choline, and decamethonium, where membrane potential falls were reported (*3*), but usually above the critical level of 52 to 57 mv.

A much greater degree of depolarization in the end-plate region, which, from Burns and Paton's work with external electrodes (*1*), was to be expected with microelectrode recordings, was not found for succinylcholine, acetylcholine, choline, or decamethonium. Nor does the theory of Thesleff that these depolarizing blocking agents decrease the sensitivity of the end-plate regions (*2*) indicate that a conduction block to direct muscular excitation would be expected.

Our findings suggest that the excitation mechanism of the membrane is interfered with or the membrane-contractile link (*6*) is blocked. The lowering of membrane potential is probably a coincident phenomenon with a generalized membrane action of succinylcholine on the muscle fiber (*7*).

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24 February 1960

Deuterium Analysis—a Simple and Precise Method

Abstract. By means of reaction with calcium hydride in a generator of simple design, the water samples are converted into H_2 and HD. With hydrogen as carrier gas, the greater thermal conductivity of HD produces a peak whose size is linearly related to the deuterium content of the original water.

Described below and depicted in Fig. 1 is a simple, inexpensive apparatus which analyzes water samples (without special purification) for deuterium content with a precision that is generally better than the mass-spectrometer and falling-drop procedures currently in use (*1*).

The mixture of HOH, DOD, and HOD (about 0.1 ml) contained in reservoir bulb *B* (Fig. 1) is allowed to drop slowly onto granules of calcium hydride contained in cartridge *C* suspended in evacuated tube *A*. This generates a mixture of only H_2 and HD and since the separation factor (*2*, *3*) for the reaction of mixed waters with calcium hydride is nearly 1.00, the HD content of the gas corresponds almost exactly to the deuterium content of the water sample. Three-way stopcock *D* connects the gas generator to double-arm manometer *E* and the gas sampler of the gas chromatography equipment *F*. After evacuation of the manometer-sampler system, the system is sealed off with stopcock *G*, through which the system is connected to a vacuum pump. The gas mixture from *A* is now introduced through *D* into the evacuated sampler and manometer and the pressure throughout is adjusted to atmospheric pressure by manipulation of the leveling bulbs. The mixed gases are now released into the carrier stream of the chromatograph.

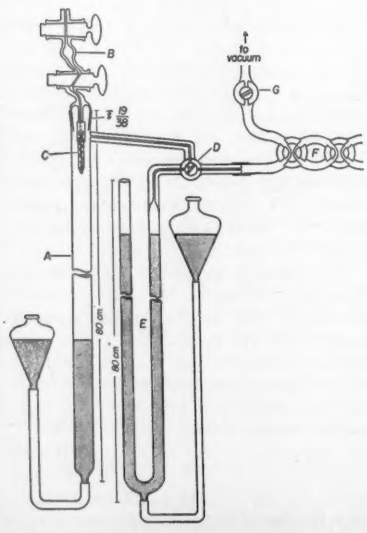

Fig. 1. Apparatus used to convert deuterium into H_2 and HD.

No attempt is made to separate hydrogen from HD (*4*). Instead, the recording katharometer of the chromatograph is used to measure the well-known (*1*) difference between the thermal conductivity of HD and that of hydrogen, the function of the column (Burrell High Activity Charcoal) being to separate volatile impurities from the hydrogen isotopes. Hydrogen is used as the carrier gas so that the size of the peak traced by the recorder depends only on the quantity of HD in the gas sample. This gives the method great sensitivity and we have detected D_2O at twice the background level (0.017 mole percent).

From 0 to 10 mole percent deuterium—the region of greatest interest for tracer studies—the relation of peak height to mole percent deuterium in the original water is strictly linear (*3*) (Fig. 2). Series of replica samples analyzed during the same day often agree within an estimated standard deviation of 0.3 (relative) percent while replicas analyzed on different days usually have a standard deviation of 0.5 to 1.0 percent, depending on deuterium content. Thus our results compare favorably with conventional methods which have a "precision" of 0.5 to 3 percent (*1*). We find that the calibration line holds within the above error over the life of a tank of hydrogen (about a week), if the hydrogen flow is left undisturbed night and day and correction is made for change in atmospheric pressure. Since the calibration curve is a straight line passing through the origin (by least squares), a single run with a standard solution serves for recalibration. Analytical precision may be improved by recalibrating immediately after running an unknown using a standard solution of nearly the same deuterium content. We are currently developing the sensitivity and precision of our method and expect that both may be improved considerably.

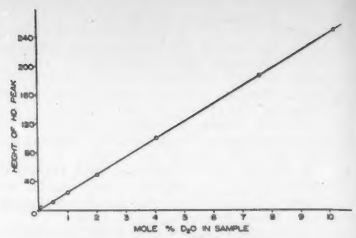

Fig. 2. Each peak height shown on this curve is an average taken from the heights of 6 to 12 curves obtained from 2 to 4 samples run on 2 to 4 different days. They have been corrected for changes in barometric pressure. Peak heights for this system are more reproducible than peak areas measured with a planimeter.

A standard Burrell Kromotog K-2 was used with a hydrogen flow of 40 ml/min, a cell current of 310 ma and a 2½-m column, at room temperature. A 20-ml gas sampler thermostatted at 50°C was employed in order to get large peaks for the less concentrated heavy water solutions. Better results for solutions with more than 2 mole percent of heavy water can be obtained by using a smaller sampler. Indeed, a great advantage of the method is the flexibility made possible by the use of different-sized gas sampler chambers for different HD concentrations, which permits the use of high recorder sensitivity and high precision over the whole H_2–HD range from 0 to 100 percent except at the extreme ends. The standard solutions used for the calibration line were prepared from deaerated, distilled water and deuterium oxide (General Dynamics, 99.9 percent). The exact deuterium content of every sample was established with a 25-ml pyknometer (*1*) at 25°C.

Reproducibility depends mainly on the freshness of the calcium hydride surface (Metal Hydrides, Inc.), the constancy of carrier gas flow, and the scrupulous avoidance of leaks during evacuation. The latter are easily detected both with the manometer and by the appearance of air peaks on the recording (*5*).

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Soc. **80**, 2909 (1958); C. O. Thomas and H. A. Smith, *J. Phys. Chem.* **63**, 427 (1959)] have approached G. L. C. deuterium analysis by separation of the isotopes. Since this involves partitioning the deuterium under two peaks rather than converting it to one measurable species, and usually requires conditioning of the stationary phase between samples, we feel that our method is inherently better, precisely because we do *not* separate the gases.

5. We thank the National Science Foundation for supporting this work, and Mr. Lloyd Guild of the Burrell Corp. for advice. This report was presented 2 March 1960 at the Pittsburgh Conference on Analytical Chemistry and Applied Spectroscopy.

23 February 1960

Inheritance of a Serum Protein in Swine

Abstract. A study of polymorphism of the starch-gel electrophoretic pattern for one of the blood serum proteins in swine (tentatively designated protein B) reveals that it is controlled by a single pair of alleles exhibiting partial dominance. *BB* genotypes appear to have twice the amount of protein B that the *Bb* genotype has, while *bb* genotypes show no evidence of the protein. Present indications are that the Yorkshire and Landrace breeds differ in the frequency of these genes.

Inherited variations of serum proteins in human beings have been studied by Smithies and Walker (*1*) and in cattle by Smithies and Hickman (*2*) and by Ashton (*3*). Starch-gel electrophoresis of pig serum was reported by Ashton (*4*) without any genetic study of the polymorphisms observed.

In this investigation, serum samples from all the parents involved in 100 litters and from random samples of progeny in these litters were subjected to vertical starch-gel electrophoresis (*5*), and the resulting electrophoretic patterns were related to pedigree information in order to determine whether the polymorphism observed was under genetic control. Blood was collected from the ear veins on sows and boars, and by cardiac puncture from 28-day-old pigs. Serum samples were stored at −25°C.

The vertical starch-gel electrophoresis technique used in this study was essentially that of Smithies (*6*, *7*), with the following differences. The buffer was made up with 125 ml of 0.2*M* tris(hydroxymethyl)aminomethane (Fisher) to which was added 62.5 ml of 0.1*N* HCl and 312.5 ml of distilled water. Seventy grams of Starch-Hydrolysed (Connaught Medical Laboratories, Toronto) and 500 ml of Tris buffer were used to prepare each gel. The electrophoresis was carried out at a voltage gradient of 5 volt/cm for 17 hours.

After staining and destaining, gels were photographed on 4 by 5 inch Kodak Verichrome Pan film with a Wratten filter F. Contact prints were used as a permanent record of each gel.

Figure 1 illustrates the types of electrophoretic patterns which were observed for the protein tentatively designated as protein B.

The analysis of the data gathered to date with respect to protein B is presented in Table 1. As indicated by the probability values, χ^2 tests support the hypothesis that phenotypes I, II, and III result from genotypes *BB*, *Bb*, and *bb*, respectively. The poorest fit was noted for progeny from the II × I matings. This suggests the possibility of a semilethal interaction between a *Bb* genotype in the progeny and a *BB* genotype in the dam.

In order to examine this possibility further, a heterogeneity χ^2 test was carried out for the progeny distributions resulting from the I × II and II × I mating classes. The results ($\chi^2 = 1.371$, $P > .20$) indicated that both mating classes were likely samples from a single population of matings which produces segregation in a 1:1 ratio. Further data will be required to determine whether this deviation has any real significance. When progeny totals from I × II and II × I mating classes are pooled, the fit to a 1:1 ratio is quite reasonable ($\chi^2 = .371$, $P > .50$).

Among the 100 litters involved in this study, 38 were Yorkshire × Yorkshire, 16 were Yorkshire × Landrace, 32 were Landrace × Landrace and 14 were Landrace × Yorkshire (males are identified first). Of particular interest is the distribution of genotypes among sows of each of the breeds (Yorkshire 22 *BB*, 27 *Bb*, 3 *bb*; Landrace 46 *BB*, 2 *Bb*, 0 *bb*).

The application of a χ^2 test for uniformity of genotype distributions among sow herds ($\chi^2 = 33.093$, $P < .01$) indicates that the genotype distributions, and therefore the gene frequencies, are significantly different. This is particularly interesting since the Landrace sows represent samples from 29 different Ontario breeders and the Yorkshire sows are descended from a wide sample of dams and 15 unrelated boars purchased from 12 different Ontario breeders during the past 3 years. The distribution of genotypes among Yorkshire boars involved in this study was 2 *BB*, 3 *Bb*, 0 *bb*, and among Landrace boars, 4 *BB*, 0 *Bb*, 0 *bb*. It is tentatively concluded that the Yorkshire and Landrace breeds differ markedly in frequency of the genes controlling development of serum protein B (*8*).

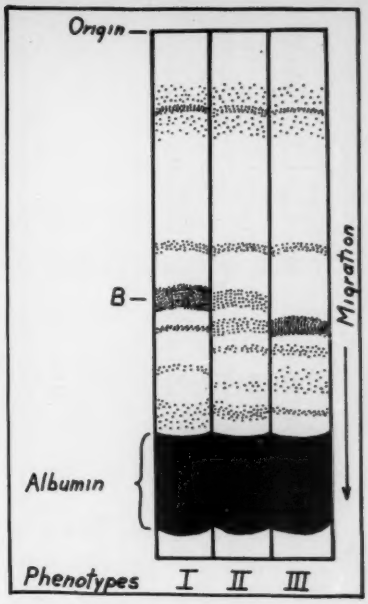

Fig. 1. Diagrammatic illustration of electrophoretic patterns showing the variants observed for protein B. Additional variation to that illustrated has been observed for other proteins.

Table 1. Distribution of observed progeny phenotypes (o) from various mating classes and those expected (e) on the hypothesis that phenotype I = *BB*, phenotype II = *Bb*, and phenotype III = *bb*.

Item	Progeny phenotypes I (*BB*)	II (*Bb*)	III (*bb*)	*P* of χ^2
	Mating class I × I			
o	109			
e	109			
	Mating class I × II			
o	25	26		
				>.80
e	25.5	25.5		
	Mating class I × III			
o		7		
e		7		
	Mating class II × I			
o	34	24		.10–
e	29	29		.20
	Mating class II × II			
o	24	47	27	
				>.80
e	24.5	49	24.5	
	Mating class II × III			
o		3	1	
e		2	2	
	Totals			
o	192	107	28	
				>.80
e	188	112.5	26.5	

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5. I wish to thank W. Zeitz for his technical assistance in the electrophoresis work involved in this study.
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7. I wish to express my thanks to O. Smithies for his interest and advice on vertical starch-gel electrophoresis.
8. This report is contribution No. 16, Animal Research Institute, Research Branch, Canada Department of Agriculture.

15 February 1960

Letters

On the Origin of Life

Many papers have appeared in recent years, several in *Science*, on the topic of the origin of life. I have been somewhat disturbed to note the ever-increasing tendency, especially among biochemists, to identify the event of the origin of life with that of the origin of enzymes or of deoxyribonucleic acid. It appears to me that our greatly increased rate of discovery of facts pertaining to the chemistry of hereditary mechanisms and the relationship of enzyme constitution to genotype has led to a distortion of the basic problem and misled many into this identification.

I do not propose to enter here into a discussion of the problem of the origin of living systems and the possible importance of cyclic factors, which I have treated elsewhere [*Am. Naturalist* **81**, 161 (1947)], but should like, rather, to present an analogy which may serve to make my point.

Consider the problem of the origin of far-flung industrial corporations. To the untrained observer the problem of the origin of such structures might perhaps resolve itself into that of the origin of corporation buildings, machinery, raw materials, capital, workers, sales outlets, and so on. The trained observer would seek his answer in a study of the history of corporations. He would correctly conclude that the corporate giant often derives from the humble garage or basement workshop in which the inventor experiments in his off hours to develop a new product or process. He would then trace the slow process of corporate evolution through the small shop of two or three employees, to the modest factory, to the giant factory, to many factories, and so on and on to the industrial complex with its directing board and many stockholders.

Let us suppose that we can no more trace the history of corporations than we can directly trace the history of life. The trained observer might instead insinuate himself into the inner workings of the corporation until he breached the inner sanctum—the board of directors. He would then discover that the board of directors is the keystone of corporate policy and action, varying the company program, product emphasis, and even the corporate structure in response to changing economic conditions and opportunities, electing the officers who see to the fine details of operation, and so on, and, indeed, that every action of the corporate enterprise traces directly or indirectly to the make-up and actions of the board—to the corporate DNA. In his new familiarity with the manifold operations of the all-powerful board and the consequences of these operations, the solution to the problem of the origin of corporations might suddenly come upon him; dazzled by his suddenly acquired knowledge, he might conclude hastily that the problem of the origin of corporations was to be identified with the problem of the origin of boards of directors!

J. LEE KAVANAU
Department of Zoology,
University of California, Los Angeles

Linear and Higher-Order Curves

In Fig. 1 of the article "Investigations of natural environmental radiation" [*Science* **131**, 903 (1960)] by L. R. Solon *et al.*, there is presented a straight-line regression on the relationship between barometric pressure and the log of radiation level. Even a casual inspection of this graph indicates that a better empirical description would be given by a second-degree curve. This may be inferred from the systematic deviation of the observed points from the fitted line.

As a simple description of the relationship within the range of observations, it would make little difference, but since this line is used for extrapolation, considerable error may result. For example, the 3.8 μr/hr extrapolation for cosmic-ray ionization intensity at sea level for New York City might result in an estimate of 5 μr/hr (or a difference of about 33 percent) if a curved line were used.

MARVIN GLASSER
44 Buswell Street,
Boston, Massachusetts

In his letter Marvin Glasser suggests that a better fit to the data in Fig. 1 of our article could have been achieved by using a higher-order polynomial than the linear regression exhibited.

I agree that a quadratic or higher-degree polynomial would constitute a closer fit to the empirical data. (In fact, since the curve is based on 19 experimental points, an 18-degree polynomial exists which would fit the data perfectly).

I disagree that using a higher-degree polynomial would result in a curve that would be better for extrapolation.

Further, I submit that if a set of empirical points actually obey a linear law, any higher-degree curves—whether they fit the data better or not—almost certainly would be worse for extrapolation. Extrapolation of such a linear relationship depends simply on the slope

of the regression line. If extrapolation from the higher-degree curve outside the empirical range gave the same numerical result as a linear least-squares fit, it would merely be fortuitous.

The linear fit is especially suitable for our airplane data. It corresponds physically to an exponential attenuation mechanism well established in cosmic radiation-absorption processes. A quadratic (or higher-degree) fit would not be justified by the physics of the situation.

LEONARD R. SOLON
Health and Safety Laboratory, New York Operations Office, Atomic Energy Commission, New York, N.Y.

Types and Name-Bearers

Shenefelt [*Science* **130**, 331 (1959)] has suggested that proposal of a new species should be primarily by description of a single specimen, the type ("holotype"). Schopf [*Science* **131**, 1043 (1960)] has objected, mainly on these grounds: types are not typical; description of a specimen does not describe or define a species; and the proper function of a type ("holotype") is solely that of name-bearing. I entirely agree with Schopf, and I made the same points at greater length 20 years ago [*Am. J. Sci.* **238**, 413 (1940)]. Since then this attitude has become widespread but, as Shenefelt's note shows, not universal.

Schopf proposes that a type as name-bearer be called "nomenifer." The combining form of *nomen* is *nomin-*, and the word should therefore be *nominifer*. I long since proposed the Greek-derived equivalent *onomatophore*. There is no particular reason to prefer Greek or Latin derivation, but current type terminology is derived from Greek.

I do not myself regularly use the term *onomatophore*, and I do not know of anyone who does. *Nominifer* is not likely to fare better. The term *type*, with all its confusing connotations, is too strongly entrenched in taxonomy. Codes of nomenclature require designation of types, under that name, and there is little or no chance of eradicating that usage.

The onomatophore or nominifer is *the* type of modern taxonomy. Additional designations such as holotype and paratype only perpetuate the idea that types can serve other than nomenclatural functions and hence can be of more than one kind.

G. G. SIMPSON
Museum of Comparative Zoology, Harvard University, Cambridge, Massachusetts

Meetings

Neuropathology

The Commission for Neuropathology was founded by invitation in the Salpêtrière Hospital in Paris on 24 October 1959. At this meeting were present Ludo van Bogaert and Pearce Bailey, president and secretary-general, respectively, of the World Federation of Neurology.

The following neuropathologists had been invited and became charter members of this commission: J. Bertrand (France); E. Christensen (Denmark); P. B. Diezel (Germany); W. Girard (France); W. Haymaker (United States); A. C. Løken (Norway); F. Lüthy (Switzerland); W. H. McMenemey (Great Britain); E. Osetowska (Poland); G. Peters (Germany), who was unable to attend the meeting; F. Seitelberger (Austria); P. Sourander (Sweden); J. O. Trelles (Peru); and W. J. C. Verhaart (Netherlands).

F. Seitelberger was elected secretary of the commission. The permanent secretariat has its headquarters at the Institute of Neurology of the University of Vienna (Obersteiner Institut), Schwarzspanierstrasse 17, Vienna 9, Austria.

The commission claims independence for neuropathology as a proper medical discipline and emphasizes the necessity of independent positions for full-time neuropathologists. Therefore the commission will examine the situation of neuropathology and neuropathologists in various countries and consider further action for protecting the neuropathologist's basic interests. In international congresses of the various neurological disciplines, the competence of neuropathology is to be recognized in an appropriate way.

Moreover the commission will be interested in preserving and caring for the valuable neuropathological collections in various places. It will try to make it possible for all interested scientists to use these collections.

One of the main tasks of the commission will be that of effectively promoting scientific activities in the field of neuropathology. For that purpose it is proposed that information be exchanged and personal contacts be arranged within the field of neuropathology and between neuropathology and related and basic scientific disciplines to develop suitable methods of investigation.

Finally, the commission is to be a consultative instrument within the World Federation of Neurology—for example, to promote important projects of investigation.

After a thorough discussion it was decided that an international journal of neuropathology should be published, to be issued in several languages.

The secretary of the Commission of Neuropathology will endeavor to keep the various medical journals informed about the activities of the commission. He welcomes all relevant suggestions from his colleagues interested in this project.

F. SEITELBERGER
Obersteiner Institut, Vienna, Austria

Forthcoming Events

June

22–25. Society of Nuclear Medicine, Estes Park, Colo. (T. P. Sears, V.A. Hospital, Denver 20, Colo.)

25–5. First Intern. Cong. on Automatic Control, Moscow, U.S.S.R. (R. Oldenburger, Mechanical Engineering Dept., Purdue Univ., Lafayette, Ind.)

26–1. American Physical Therapy Assoc., Pittsburgh, Pa. (Miss J. Bailey, 157 N. 79 St., Milwaukee 13, Wis.)

26–1. American Soc. for Testing Materials, Atlantic City, N.J. (R. J. Painter, 1916 Race St., Philadelphia 3, Pa.)

26–1. Mass Spectrometry, 8th annual, Atlantic City, N.J. (V. H. Dibeler, National Bureau of Standards, Washington 25)

26–1. National Education Assoc., Los Angeles, Calif. (W. G. Carr, 1201 16 St., NW, Washington 6)

26–2. American Physical Therapy Assoc., Pittsburgh, Pa. (Miss L. Blair, 1790 Broadway, New York 19)

27–29. Military Electronics, 4th natl. conv., Washington, D.C. (C. M. Crenshaw, Dept. of Army, Office of the Chief Signal Officer, R. & D. Division, SIGRD-2, Washington 25)

27–29. Status of Problems of Molecular Structure, symp., Seattle, Wash. (P. C. Cross, Dept. of Chemistry, Univ. of Washington, Seattle 5)

27–30. Coherence Properties of Electromagnetic Radiation, conf. (by invitation), Rochester, N.Y. (E. Wolf, Optics Institute, Rochester Univ., Rochester)

27–30. Institute of the Aeronautical Sciences, Los Angeles, Calif. (R. R. Dexter, IAS, 2 E. 64 St., New York 21)

27–30. National Assoc. of Power Engineers, annual conv., San Francisco, Calif. (E. J. Schuetz, NAPE, 176 W. Adams St., Chicago 3, Ill.)

27–1. International Assoc. for Bridge and Structural Engineering, 6th cong., Stockholm, Sweden. (P. Lardy, IABSE, Ecole Polytechnique Fédérale, Zurich, Switzerland)

27–1. Reading Conf., 2nd annual, Syracuse, N.Y. (R. A. Kress, Reading Center, Syracuse Univ., Syracuse 10)

28–1. American Home Economics Assoc., Denver, Colo. (Miss M. A. Warren, School of Home Economics, Univ. of Oklahoma, Norman)

29–1. Health Physics Soc., 5th annual, Boston, Mass. (E. E. Anderson, Health Physics Div., Oak Ridge National Laboratory, Oak Ridge, Tenn.)

July

3–5. American Assoc. of Colleges of Pharmacy, Boulder, Colo. (G. L. Webster, College of Pharmacy, Univ. of Illinois, Chicago 12)

4–8. Polarization Phenomena of Nucleons, symp., Basle, Switzerland. (K. P. Meyer, Physikalisches Institut der Universität Basle, Klingelbergstr. 82, Basle)

5–9. Goiter Conf., 4th intern., London, England. (J. C. McClintock, 149½ Washington Ave., Albany 10, N.Y.)

6–15. Entomological Conf., 7th Commonwealth, London, England. (Commonwealth Inst. of Entomology, 56 Queen's Gate, London, S.W.7)

10–14. Pan American Tuberculosis Cong., 12th, Bahia, Brazil. (F. D. Gómez, 26, de Marzo, 1065, Montevideo, Uruguay)

11–12. Response of Materials to High Velocity Deformation, conf., Estes Park, Colo. (AIME, 29 W. 39 St., New York 18)

11–15. British Dental Assoc., annual, Edinburgh, Scotland. (Secretary, British Dental Assoc., 13 Hill St., Berkeley Sq., London, W.1, England)

11–15. Royal Medico-Physiological Assoc., annual, London, England. (A. B. Monro, 11 Chandos St., Cavendish Sq., London, W.1)

11–16. Inter-American Nuclear Energy Commission, 2nd meeting, Petropolis, Rio de Janeiro, Brazil. (J. D. Perkinson, Jr., Inter-American Nuclear Energy Commission, c/o Pan American Union, Washington 6)

(See issue of 20 May for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.*

■ ATMOSPHERIC PARTICLE MONITOR presents on a strip-chart recorder a record of aerosols present in the range from 0.03 μ to any desired upper limit. Counting rate is 1000 particles per minute with a 1-percent coincidence loss at a standard flow rate of 100 cm³/min. The record is divided into 15 subranges that are recorded in sequence at intervals that can be predetermined in length. The unit is continually self-calibrated. An alarm system can be set to operate if concentration in any of the subranges exceeds a predetermined value. (Royco Instruments, Inc., Dept. Sci569, 365 San Antonio Rd., Mountain View, Calif.)

■ CURRENT-SURGE GENERATOR furnishes single half-wave sinusoidal surge currents adjustable between 5 and 75 amp at a maximum repetition rate of four per minute. Provision is made to monitor the output with an oscilloscope through a 50-mv shunt. A synchronization signal is provided. (Wallson Associates, Dept. Sci570, 912 Westfield Ave., Elizabeth, N.J.)

■ TEMPERATURE SENSOR is designed for in-wall locations in steel structures having a section thickness ¼ in. or greater. Chromel-Alumel is the junction material offered as standard. Body lengths are available in ⅛ in. increments from ¼ to 3½ in. Thermocouple junction location is determined to ±0.002 in. or less in standard increments of ⅛ in. from 0.001 in. (from heated surface) to 0.005 in. (from unheated surface). (American Standard, Dept. Sci571, 369 Whisman Rd., Mountain View, Calif.)

■ RESOLVER TEST STAND is a shaft-rotation-positioning device consisting of an optical coincidence reading system said to have ±2-sec accuracy, a rack and gear for positioning, and an adapter plate and coupling. Angular position is read directly to the nearest second of arc. (W. and L. E. Gurley, Dept. Sci-573, Troy, N.Y.)

■ CATHODE RAY TUBE is designed for low deflection voltage, less than 15 v/in., to permit design of compact equipment and transistor circuitry. Light output is said to be high in relation to the accelerating voltage used (500 volts). Post acceleration is used to minimize distortion. (Allen B. DuMont Laboratories, Inc., Dept. Sci574, Clifton, N.J.)

■ REFERENCE-VOLTAGE SOURCE provides a-c voltage output from 1 to 501 volts r.m.s. adjustable in 0.1-volt steps and by multiturn potentiometer within steps. Output is 5 amp or 25 watts, whichever is less. Frequencies selectable are 60, 400, and 1000 cy/sec. Accuracy is said to be ±0.1 percent or ±5 mv, frequency stability 1.0 percent, and harmonic distortion less than 0.3 percent. Output impedance with a constant resistive load is approximately 0.01 ohm. (Cohu Electronics Inc., Dept. Sci572, 5725 Kearny Villa Rd., San Diego 12, Calif.)

■ PHOTOMETER measures the difference in intensity of two light sources or measures and records each intensity independently. Sensitivity of each scale is adjustable over a ten to one range. Accuracy is said to be ±0.5 percent in each channel and stability ±0.1 percent per channel per day. Five ranges from 0.001 to 10 μa full scale are provided. Commercially available phototubes or multiplier phototubes may be used. (Eldorado Electronics, Dept. Sci575, 2821 Tenth St., Berkeley 10, Calif.)

■ OSCILLOSCOPE CAMERA, for 5-in. oscilloscope, is of modular design for adaptability to a wide range of object-to-image ratios. Accessories include a binocular viewing hood; adaptor to record up to ten traces on a single frame; 35-mm pulse camera; 35-mm continuous motion camera; data chamber with watch, platen, and counter; data card; external focusing control; and electric shutter actuator. (Beattie-Coleman Inc., Dept. Sci566, 1090 N. Olive St., Anaheim, Calif.)

■ DISPLACEMENT TRANSDUCERS of differential-transformer types are said to have nonlinearity of 0.3 to 0.4 percent over ¾ of full-range length. Six models range in length from 1½ to 17 in. with diameter 0.5 in. Operating temperature range is −100° to +350°F. Operating frequency is 400 cy/sec. Repeatability is said to be ±0.01 percent. (Pacific ElectroKinetics, Dept. Sci577, 329 S. Vermont Ave., Glendura, Calif.)

■ TIMER uses a filtered power supply feeding an *RC* network through a transistor to operate a relay at the end of the timing cycle. No warmup time is required. The instrument is rated at 3×10^9 operations, with accuracy said to be ±1 percent. Contacts are rated 15 amp at 115 volts a-c. Direct-current models are available. Full scale ranges are available from 1 to 120 sec. (Automatic Timing and Controls Inc., Dept. Sci581, King of Prussia, Pa.)

JOSHUA STERN
National Bureau of Standards, Washington, D.C.

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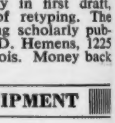

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How Boundary Layer Control Reduces Friction Drag on Aircraft in Flight, Can Increase Range 50% to 100%

by Dr. Werner Pfenninger

Dr. Pfenninger, internationally renowned for his work in laminar flow and low drag coefficients at the Federal Institute of Technology in Zurich, joined Northrop in 1949. Today he heads the scientific and engineering team working on Boundary Layer Control Research at Norair Division, Northrop Corporation.

The boundary layer is a very thin stratum of air that surrounds an aircraft in flight. Lying directly on the aircraft, it is formed by air that is slowed by the passage of the aircraft surface and flows more slowly than the free stream velocity of the air around it. Turbulence results. This increases air friction—waste friction drag (not to be confused with *induced* drag associated with lift).

Norair Low Drag Boundary Layer Control prevents this turbulence by smoothing the flow of air. BLC draws part of the turbulent air through slots in the aircraft skin with a suction compressor and exhausts it in the direction of thrust through the aft fuselage or the nacelle tailpipes. On wings and empennage this reduction can increase the aircraft's range or endurance 50 per cent. If BLC is applied to most of the airplane, this increase can go as high as 100 per cent.

BLC's reduction of friction drag increases endurance missions, flight efficiency, and amount of pay load. It reduces fuel consumption to the point where aircraft can remain on station for long periods of time. It reduces operating costs.

Any type of large, long-range aircraft can be quickly adapted to incorporate Norair's BLC aircraft skins. Cost of BLC application is approximately 10 per cent of the total airplane.

For military applications, BLC means advances in aircraft performance as an air-to-surface missile carrier on air defense missions, airborne early warning systems, anti-submarine warfare, tactical air support, intercontinental communications link. On logistic missions, BLC allows nonstop flights without refueling to decrease dependence on overseas bases. On commercial aircraft BLC's increased range, payload and cost advantage mean a remarkably increased ability to carry air freight. For flights of 2000 miles or more, direct ton-mile cost can be reduced by 10 to 50 per cent. On passenger flights, long-distance nonstop flights would be possible to any point in the world. For nuclear-powered aircraft of the future, BLC means smaller power plants with less heavy and costly radiation shielding that makes a lighter airplane possible.

Northrop Corporation—the only American aerospace company to conduct extensive research in the field—has pioneered Low Drag Boundary Layer Control for more than ten years. Today Norair has conducted more than 250 laminar flow flights with an F-94 jet fighter. Full laminar flow has been obtained in wind tunnel tests on varying shapes at varying speeds from low subsonic to high multi-sonic Mach numbers.

For the scientist and engineer at Norair, design, manufacture, and flight testing of BLC offers a gratifying opportunity to create advances in a new field. The specialist enjoys the advantages of working with the facilities of the entire Northrop Corporation in an atmosphere where management encourages, recognizes, and rewards individual achievement.

Current papers by Dr. Werner Pfenninger include:

Design Considerations of Large Subsonic Long-Range Transport Airplanes with Low Drag Boundary Layer Suction.

Design Considerations of Propulsion Systems for Low Drag Boundary Layer Control Airplanes Cruising at High Subsonic Speeds.

For copies of these papers and additional information about Northrop Corporation, write:

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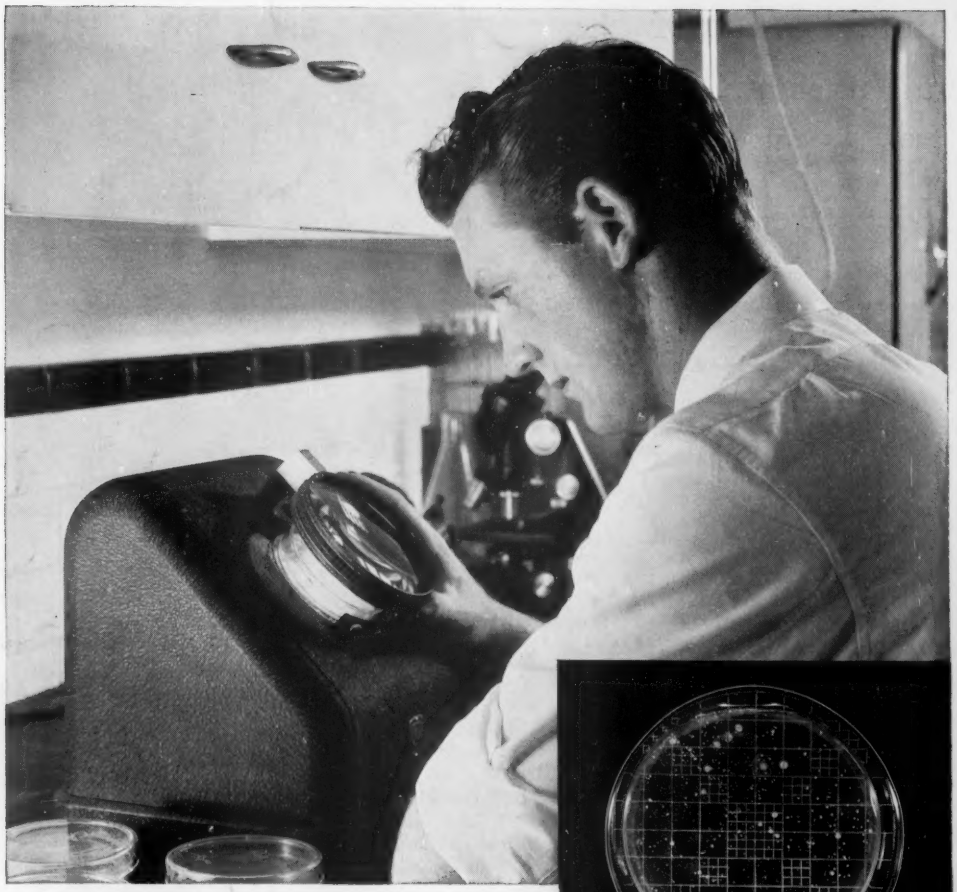

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